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ART. XVII.—*The Atlantic Cable;* by GEORGE MATHIOT, Electrotypist of the U. S. Coast Survey.—(In a letter to Prof. A. D. BACHE, Supt. of the U. S. Coast Survey, dated Coast Survey Office, Washington, Sept. 11, 1858.)

As the Atlantic Cable has been laid for more than a month and the line is not yet opened to the public, it is evident that there is some great difficulty in the way. According to the newspapers the trouble is in the *recording* instruments. I fear, however, that such is not the true cause of the delay. I investigated the electrical conditions which would obtain in the cable as soon as I had procured a specimen of the material, and ascertained the electrical views entertained by the managers. From that investigation I positively predicted that the cable would be a failure unless other views of the generation and distribution of electricity should obtain in the subsequent management.

It is greatly to be regretted that the counsel of certain electricians prevailed in the construction of a cable having so small a conductor, and so thin an insulation. But now that it is laid, such as it is, at such immense labor and expense, in conjunction with fortuitous circumstances on the ocean, it becomes us to summon every aid of science and independently of theory to interrogate her *experiments* and hearken to the infallible directions which these give through the unerring calculus.

Experiment shows that an immense amount of electricity is disposed on the sides of the conductor before the current mani-

fests itself through it. Theorists attribute this delay solely to the circumstance of the electricity being thus disposed, and so leave us without a remedy. The investigations of Ohm, however, tell us that the time of conveying a given quantity of electricity between any two points is determined by the tension at those points and the resistance between, and hence we infer that if the sides of the wire must first be filled before any will pass through, then the time will depend on the quantity required, the tension at the source and the goodness of the conductor. Why should the mere circumstance of *where* the electricity is disposed cause the delay in its disposal?

I considered the electrical conditions for a submerged conductor on the first proposal for an ocean telegraph, and was so fortunate as to have a paper containing some of my deductions, published in the Coast Survey report for 1855. In that paper I described a voltaic experiment which proves that a battery requires *time* to generate the *quantity* of electricity required to produce the tension needed to overcome a certain resistance, and inferentially, that time is required for the *generation* as well as for the distribution of electricity. If you will turn to that paper you will there see foreshadowed all the present difficulties of the cable; so fully am I persuaded of the correctness of the principles there given, that I would confidently undertake to charge the cable in a very short time or extend the time of charge many minutes. In the limits of a letter I will not be able fully to lay open the principles which I conclude should govern the electricians of the cable, and fearing to intrude on your time I will proceed rapidly to elucidate the main points and describe what I would propose.

The difficulties of the "static induction" are all previsedby Ohm, and I cannot but wonder that the European electricians did not thence comprehend it; the delay of the current I thence anticipated before experiment had exhibited it. The fundamental principle of Ohm is that the quantity of electricity which passes between two contiguous particles in a given time is proportional to the difference of tension between them. Regardless of this fundamental idea of the distribution, the electricians of the cable have sought to work it as a simple problem of the conduction of a quantity through, as on fully insulated conductors (those suspended in air) instead of considering it as the problem of the charge and discharge of a Leyden jar (an unlimited number of momentary conducting lines): thus regarding the conditions of the flow of the current through the conductor, and ignoring the circumstances under which those conditions are established.

The *quantity* of electricity which a Leyden arrangement takes as a charge, is proportional to the intensity of the source, the

extent of surface, and the thinness and nature of the insulation, (the dielectric); the *time* required for charge is determined by the quantity to be conveyed, the resistance of the conductors and the difference of the tensions, and the further resistance due to a certain constant, dependent on the nature of the electric medium, (its coefficient of elasticity,) which is wholly undetermined so far, and probably can not be handled before we obtain conducting lines of many thousand miles in length, but which is so small that it may be disregarded in all practical applications of electricity: in which we may make the expression of the time the ratio of a fraction $\left(\frac{T-t}{Q+R}\right)$ where T, t, are the tensions, Q the quantity required, and R the resistance, and we certainly have the means of increasing or diminishing it to the extent of our ability to handle the generating electrical apparatus, up to the point of fusion of the conductor. By Ohm's principles, and also by the demonstrated experiments of Coulomb, when a surface charged to any given intensity is brought in contact with another surface the electricity is distributed between them and the tension falls in proportion to the combined surfaces divided by the first surface; the time to convey a given quantity of electricity into a given surface will depend therefore on the *extent* of the first charged surface and the tension of the charge, with the resistance to conduction. As the quantity of electricity flowing from S to s at any moment after contact will be $\frac{T-t}{R}$ and the time for passing the whole quantity or producing equilibrium $\left(\frac{T-t}{R}=0\right)$ will be dependent on the quantity to be passed, and as this latter is $\frac{S^2T}{S+s}$, it is evident that the whole time for charge is dependent on the value for S as well as of s and the tension of the source; and that therefore the time in the problem of the conveyance of a given quantity of electricity from a charged surface S to a recipient surface s will be a decreasing function of S.

The Atlantic cable is a Leyden battery of over four acres of coated surface, (the surface of the conductor). The electrical arrangements for working the cable I cannot learn, but suppose they use an ordinary intensity battery of small pairs, probably having terminal plates of but a few square inches of surface: hence we have the ratio of S to s as millions almost; when this small charged surface contacts with the great one, its tension is lowered enormously, the difference of tensions is almost destroyed, consequently the quantity carried in a given time is proportionally diminished $\left(\frac{T-t}{R}=Q;\ \text{when } T-t=0 \text{ then } Q=0\right)$.

Is it any wonder under such circumstances that four or even ten seconds, as rumor states it, is required to convey into the cable the quantity of electricity required to raise the tension to the point due to the great number of elements in the voltaic generator they employ.

The electricians of the cable seem to have concluded that the tension of the terminals of the battery would continue after contact, although the demonstration of Ohm, and in fact all the analysts of the distribution, have shown that it would fall, and my experiment published in the report of 1855 has even made the decline visible to the senses; or they seem to have proceeded on the supposition that the battery would in all cases maintain a maximum intensity by generating the electricity faster than it could be carried: that is as though the terminals of the battery would maintain the tension exhibited in the open circuit. They appear to have been likewise regardless of the warning of Ohm, that in the case of the exterior conduction resistance being removed or greatly decreased, a certain additional resistance will arise from the mechanical obstruction of the chemical reagents and products, a resistance which must be determined not only for each electro-chemical arrangement but also for each mechanical construction and size. These are conditions however which do not generally affect the distribution, and sensibly affect the generation only when the battery is under circumstances for vigorous action; that such circumstances obtain when the contact is made with the four acres—circumstances which are almost equivalent to uniting the terminals of the battery with a short thick wire—is shown by Mr. Faraday's describing "a *rush* of electricity into the wire" at the moment of contact (see Phil. Mag. vol. vii, p. 199). In the general formula $\frac{nE}{R+r}=Q$ which is generally taken for the equation of the distribution, if we make $R+r=0$, Q becomes infinite: a condition which constitutes *detonation* and readily obtains to the amount of the matter present in the firing of certain mixed gases, guncotton, the fulminates and a few more substances, but which thank heaven is hindered from obtaining in the great majority of electrical actions (the multitudinous phenomena of nature) by the obstruction due to mechanical resistances. One consequence of the removal of the whole of the conduction resistance would be that when we touch a piece of oxydable metal, both the metal and finger should explode; happily the mechanical resistance prevents this, and even in the most energetic forms of the voltaic battery it is so great as to make the generation of very large quantities of electricity a matter of great difficulty.

To form an idea of the limiting power of this resistance I dusted pulverulent platinum over a plate of zinc of one square

foot of surface, and immersed it in a mixture of one part acid in five of water, and found that one pound of zinc was dissolved in fifteen minutes; this then was the measure of the maximum amount of electricity a battery of such size could generate in fifteen minutes; but the whole amount a practical construction of plates of such size could generate, would not be the tenth of that amount, and it is easily foreseen that whatever the amount might be, a cable of such length and favorable arrangements for "static induction" could be constructed that all the electricity which could be generated by a series of such pairs, in fifteen minutes, with least conduction resistance, could be disposed on its sides before it would constitute a charge, and consequently the charging of such cable with that battery would require more than fifteen minutes.

The whole quantity of electricity which any battery will generate in a given time under any given circumstances will be proportional to the size of the battery plates and the interior conduction resistance, and consequently the time required for any battery to charge the Atlantic cable will be in an inverse relation to the size of the plates—the number of pairs not affecting this: it merely determining the tension to which the charge rises; though on this latter depends the available quantity at the receiving end for recording.

A battery to charge the cable in the *least* time should have the plates of *unlimited* size: this would not be possible, neither would it be practical to have them bear any small proportion to the four acres to be charged. The minimum time however may be nearly obtained by using a sufficient reservoir to collect the electricity evolved from convenient sized plates and thus working the cable by means of an immense Leyden battery. For this purpose I would construct a Leyden jar of metallic sheets and varnished paper or silk, of several acres in extent, and connect the alternate metallic sheets with suitable bars for attaching the battery with the earth and cable. It will doubtless at once be perceived that it would take quite a time to charge such a Leyden jar—say eight acres—by the battery, and here it will be seen that the mask is up which has disguised the time of charge (the so-called *wave time*). To merely enlarge the battery plates somewhat above their present dimensions would doubtless obviate the difficulty to a proportional extent, but full enlargement should be made, for it will be desirable to work as rapidly as possible. With such a Leyden battery as I propose the tension which propels the electricity into the wire could at no time fall more than one third, and its effective power in charging the cable would be some thousand times greater than an arrangement of an intensity series of small pairs. In short, it is apparent that by such an immense Leyden arrangement and a sufficient battery

to charge it, the electricity would find its way through the conductor or through the insulation.

From the tone of the European publications I have been led to infer that they do not consider it possible to work the cable in a prompt and efficient manner by means of the battery. The slow and imperfect action of the Balaklava and some other cables seems to have early thrown a damper on the prospects for submarine lines, and the suspicion seems to have become conviction after the publication by Mr. Faraday of his experiments, together with those of M. Melloni and Mr. Clark; according to those experiments it is demonstrated that increasing the number of pairs in the battery, or increasing the "*intensity*," as this operation is often improperly called, does not diminish the "wave time" or the time for the electricity to flow through the conductor. By the experiments on the subterraneous wires of the Manchester telegraph, the time for the current to manifest itself through 762 miles of wire, was the same whether 30 pairs, 50 pairs, or any number of pairs up to 500 were used. If I do not mistake the tone of these, and all the subsequent papers on the wave time for submerged conductors, these experiments are regarded as decisive against the possibility of shortening the time by any modifications of the battery. At the results of those experiments M. Melloni appears to express astonishment, and considers it as opposed to the laws of Coulomb and others, and against the received ideas of *quantity* and *intensity*. Mr. Faraday appears to be disappointed however, he having predicted if I rightly apprehend his meaning, that the current from the greater number of pairs should have manifested itself more quickly than that from the lesser, and this nonconformity of the experiment with his predictions he attributes to the fact that while the *intensity* of the current was increased, its *quantity* was also increased, and considers with the increase in the number of the pairs, their *size* should have been diminished in order that only the same quantity of electricity in a more intense condition might have been permitted to traverse the conductor. From all this it is evident that those views offer but small prospect of the battery ever serving to work the submarine telegraphs. I cannot however, but express astonishment that any other effect on the wave time, from increasing the number of pairs, than that exhibited in the experiments of Mr. Clark should have been expected, considering that at the first moment of the contact of the battery with the insulated submerged conductor, the insulation in undergoing polarization is acting as a conductor; thus at the beginning of contact, the wire acts not only as a conductor of the capacity of its cross section, but also as though it had a solid section of the area of its whole surface, thus acting on the battery, as a short thick wire (in the case of the Atlantic cable

as a wire having a solid section of 185,000 sq. ft.) in which the resistance exterior to the battery may be considered as nothing, and the whole resistance to the generation of the electricity as being only that which lies within the battery: now taking Ohm's formula for the electric current $\frac{E}{R+r}$, in which E represents the electromotive force of the chemical affinity, R the resistance to conduction due to the materials of the battery, and r the resistance of the conductor which completes the circuit by connecting the poles of the battery, and considering the effect of multiplying the number of the elements we have $\frac{nE}{nR+r}$, and considering r as being indefinitely decreased by diminution of the length of the conductor, or by increase in its solid section, which are the conditions that obtain while the cable is receiving its static charge, we may consider it as $=0$, and remove it from the equation, and then we have $\frac{nE}{nR}=\frac{E}{R}$, in which it is evident a train of batteries in the conditions which obtain at the first moments of contact with the cable, can generate no more electricity than a single cell attached to the same conductor. It is here evident that the facts arrived at by Mr. Clark, so far from being a cause of wonderment, are but what the circumstances should have suggested, neither do they call for a modification of Coulomb's laws of induction, or a change of our ideas of quantity and intensity, as suggested by M. Melloni; much less do they indicate that a diminution of the size of the battery plates would hasten the current as suggested by Mr. Faraday. When a battery has the circuit open we conceive that the terminal plates have a tension proportional to the number of elements in the train, $T=nE$; and as the whole quantity of electricity they contain will be the product of their surface by the tension, $Q=TS$, it is evident that when the battery touches any inductive surface of great extent, such as that of the insulation of the cable, Q will be divided with it, and as S compared to that surface is insignificant, and as S is constant, T will fall enormously, as has been before shown for another purpose; now when the terminals of a compound battery, are connected with a stout wire, it is known that the tension falls to that of a single pair of plates, it is therefore evident that contact of the battery, with a large inductric surface, is equivalent to connecting with a non-resisting conductor. In this state of the battery it is evident that the electricity can be conveyed into the conductor only as fast as the battery can admit of the diffusion within, or bring the electricity to the terminals, in which case, as has before been shown, many pairs can generate no more electricity than a single pair, and consequently the tension will be below the maximum of a single pair,

in proportion to the smallness of the plates. Is it any wonder that with the feeble tension produced under such circumstances, such a long time is required for the battery to charge the cable?

For the sake of illustration I have considered $r=0$; such however is not strictly the case, the wire offers *some* resistance and the gutta percha offers some resistance to polarization, the tension of the terminal plates will therefore rise in proportion to this resistance until ultimately it reaches the maximum tension due to the number of electromotive elements in the train. If we consider furthermore that with any finite resistance the tension will rise in proportion to the decrease in the value of R, or that the extreme tensions will be inversely proportional to the internal conduction resistance of the battery—or if in the formula $\frac{nE}{nR+r}=Q$ we give to r a small value and diminish R by enlarging the size of the battery plates, so as ultimately to obtain $\frac{\frac{nE}{m}}{\frac{nR}{m}+r}=m\ Q$—we see plainly that by enlarging the number of electromotive elements in the battery, and at the same time increasing their size, any required effect can be produced on the cable.

From all the above it is evident that the time for any battery to charge a Leyden jar will be in a certain inverse proportion to the size of the battery plates; the charging of the cable is similar to the charging of the jar, excepting that it is complicated by the resistance of the wire, and the time will be a function of the internal conduction resistance of the source of the electricity. The general mathematical considerations must apply to every electrical action, whether it be produced by thermo-electricity, by chemical action, by magneto-electricity, by induction coils, or by the lightnings from the clouds. But as the battery is the only effective source of electricity, and as it is the cheapest also, reason calls for the construction of a sufficient battery, for working the cable in preference to all secondary contrivances.

No one at all acquainted with the subject will doubt that such an immense Leyden jar as I proposed above for passing our longitude signals, if charged to a high intensity, would more than all other things be fitted for affecting the cable. If the great jar were not discharged in charging the cable, it would continue steadily to work it in the transmission of messages; a jar that would remain thus continually charged would be a *generator* of electricity; such is a galvanic battery, and by increasing the number and size of the plates, the battery is made into such a continuously charged jar capable of producing any extent of effect.

I have not considered the fact that by the resistance of the wire of the cable, and the reaction of the parts which first receive the static charge, the flow of the electricity is broken up into waves, and that consequently the *whole* of the insulation of the cable does not perform its full inductive office at once. I have necessarily been confined to general principles and admitted conditions of electrical action, the deductions however are the same as though the whole inductive surface of the cable were simultaneously charged, for the degree of charge given to the first portion determines the rapidity with which the successive portions receive their charge.

The plan I propose for large plates or an electrical reservoir has advantages also for the *discharge*. Already it has been proposed and is practised I believe to discharge the conductor by making contact with the opposite end of the battery. As the plan I propose offers such greatly increased electrical conditions, it will correspondingly decrease the time of discharge.

I have no doubt that you will at once coincide with me in the correctness of the views of the cause of the slow working of the cable. I think you have previously perceived that a great fall in the tension must take place at the moment of contact, and the consequent importance of having large terminal surfaces to hold a sufficient quantity of electricity to maintain the tension. The expression by you of such views I take it is what Dr. Gould refers to in his report on the telegraphic operations for longitude.

I have already extended this letter to a greater length than consideration for your valuable time would justify; but that I may succeed in presenting the correctness of my views on this important matter of the Atlantic telegraph let me trespass on you for a minute longer, to show that these new views are in fact *old*, that all electricians acknowledge them in their writings, and that the phenomenon of the delay of the current is amongst the earliest experiments in electricity.

When the knob of a DeLuc's column is brought into contact with a gold leaf electroscope, the leaves diverge. If while the leaves are diverged the terminals of the pile are brought in contact, one with the inner and one with the outer coating of a Leyden jar, the leaves collapse, and the jar is found feebly charged; if the pile is now detached from the jar the leaves will begin gradually to diverge again; if however the pile is left in connection with the jar the leaves will after some time (several minutes) diverge again. Now the time between the collapse of the leaves and their regaining their divergence, is the time required for charging the jar, to the tension due the pile:—that is, it is the time required for that generator of electricity (the DeLuc's pile) to generate the quantity of electricity required to charge that jar to the tension due to the pile. Not a single

electrician I suppose could be found, who would question the correctness of this; all will admit that the time is dependent on the ability of the pile to generate the electricity, and the extent of the inductric surface (the size of the jar). But is not this phenomena precisely equivalent to the charging of the cable? is the DeLuc's column a poorer generator in proportion to the surface of the jar than an ordinary battery is in proportion to the four acres of cable surface? Every electrician who would have the problem of diminishing the time of charging the jar by the column, would have to resort to methods of increasing the activity of the pile, either by changing the nature of its elements, or increasing their surface; then why does not the same doctrine hold good in regard to charging that big jar, the Atlantic cable?

I have endeavored to demonstrate that submerged telegraph lines differ from the land lines in requiring batteries having the plates *very large*, because the electrical condition of the cable at the first moments of contact are such that require a battery capable of furnishing a current of great *quantity* as well as great intensity. I have, however, made no indication of the precise size or number of plates which should be used for working the Atlantic cable, or any other submerged conductor, with greatest speed. The limit to the rapidity of working must be determined by the conditions for carrying off the heat generated in the conduction by the passage of the electric current; and as the quantity of electricity which may be used on any cable without endangering the gutta percha insulation by the heat of the conductor can be ascertained only by experiment, it is by this means only, that the best size and number of plates can be determined for working with the greatest speed the cable will admit of.

In the passage of very large quantities of electricity into the cable the heating effect on the portion nearer to the battery will be considerable, for although the measure of the heating power of the current is the conduction resistance, and the resistance of the current is destroyed by the induction "*masking*" the electricity; yet as the portions of the cable nearer to the battery are first charged, and the conductor, after the dielectric is charged, resists as an ordinary conductor, it is evident that those portions will be heated by the current; yet the whole quantity of heat produced will be less than if no inductive action took place. Considering the whole quantity of heat generated by the oxydation of a given quantity of zinc, or the resistance to the electricity thereby generated, which is the same thing, and the greatness of the surface to carry off that heat, it is easy to foresee that a very large quantity* of electricity might be used on the

* While a quantity of electricity even *insufficient* to charge the whole cable might *rupture* it if in a state of very great tension, yet no danger of this need be apprehended from *any quantity* of electricity obtained from any easily attainable number of voltaic elements.

cable without endangering it, and consequently a high speed in transmission obtained. For a first trial on the Atlantic cable I would use a Smee battery of about 500 pairs of plates, each having 20 square feet of surface; yet it may be found on trial that plates of much larger size may be required.

Although the present cable may be incapable of working with a satisfactory degree of certainty and rapidity, even with the best electrical management, yet this would not demonstrate that there cannot be a satisfactory electrical communication across the Atlantic. Certainly I will be justified in saying that the present state of the engineering art and the experience already obtained in laying submarine conductors, can lay down a new cable capable of transmitting 100 letters per minute, if it is constructed and worked in full regard to the principles of electricity.

ART. XVIII.—*On the Variation of the Magnetic Needle at Hudson, Ohio;* by ELIAS LOOMIS, Professor of Mathematics and Natural Philosophy in the University of the City of New York.

DURING my residence at Hudson, Ohio, between the years 1837 and 1844, I made repeated observations for the purpose of determining the variation of the magnetic needle. During the summer of 1849, while employed in determining the longitude of Hudson by telegraphic comparisons with Philadelphia, I repeated my observations for the variation; and Prof. C. A. Young, of Western Reserve College, has put into my hands a very complete series of observations made by himself during the past summer. All these observations were made with a variation compass by Gambey of Paris, having a needle about 18 inches long, supported by fibres of untwisted silk, and resting in a stirrup which admits of easy reversal. It is now proposed to compare these observations for the purpose of determining the annual change of the variation. As these observations were made at different hours of the day, it is necessary to apply a correction to reduce each result to the mean variation of the month of observation. As the observations required for this purpose have never been made at Hudson, or at any place in its immediate vicinity, we must rely upon observations made at a considerable distance. The nearest station at which such observations have been made is Toronto, which is north of the parallel of Hudson. On the southern side we have observations both at Philadelphia and Washington. I have preferred the latter, since Washington is nearer to Hudson than Philadelphia; and the parallel of Hudson is exactly midway between the latitudes of Toronto and Washington.

The following table shows for six months of the year, the quantity by which the variation at the hour named in column first, differs from the mean variation for the month at Toronto. The table is derived from five years observations between 1843 and 1848, and is copied from page 90, vol. iii, of the Toronto observations.

Diurnal change of the Magnetic Variation at Toronto.

Hour.	April.	May.	June.	July.	August.	Sept.
8 A. M.	4·96 E.	5·82 E.	6·20 E.	6·26 E.	6·90 E.	4·80 E.
9 "	3·96 E.	4·18 E.	4·72 E.	4·86 E.	4·76 E.	3·00 E.
10 "	1·30 E.	0·64 E.	1·73 E.	1·78 E.	0·54 E.	0·56 E.
11 "	2·02 W.	3·06 W.	2·00 W.	1·76 W.	3·12 W.	3·70 W.
noon.	4·54 W.	5·24 W.	4·72 W.	4·24 W.	6·00 W.	6·26 W.
1 P. M.	5·98 W.	6·28 W.	6·20 W.	5·86 W.	7·22 W.	6·38 W.
2 "	5·84 W.	6·10 W.	6·26 W.	5·96 W.	6·52 W.	5·40 W.
3 "	4·98 W.	4·84 W.	5·26 W.	5·12 W.	5·04 W.	3·36 W.
4 "	3·30 W.	3·12 W.	3·76 W.	3·68 W.	2·74 W.	1·16 W.
5 "	1·66 W.	1·26 W.	1·68 W.	1·86 W.	1·02 W.	0·42 W.
6 "	0·80 W.	0·44 W.	0·62 W.	0·70 W.	0·18 W.	0·12 E.
7 "	0·32 W.	0·22 W.	0·28 W.	0·36 W.	0·13 W.	0·06 W.
8 "	0·44 E.	0·18 W.	0·32 W.	0·66 W.	0·10 W.	0·24 E.

The following table shows for the same months the quantity by which the variation at the hour named in column first, differs from the mean variation for the month at Washington. It is derived from two years observations from 1840 to 1842, and is deduced from the table on page 326 of Gilliss' Magnetical Observations.

Diurnal change of the Magnetic Variation at Washington.

Hour.	April.	May.	June.	July.	August.	Sept.
h m						
0 12 A. M.	0·97 E.	0·65 E.	0·67 E.	0·96 E.	0·82 E.	0·58 E.
2 12 "	1·81 E.	0·93 E.	0·39 E.	1·16 E.	0·99 E.	1·02 E.
4 12 "	2·42 E.	1·33 E.	1·27 E.	1·57 E.	1·26 E.	1·47 E.
6 12 "	2·75 E.	3·65 E.	4·09 E.	3·58 E.	4·03 E.	3·05 E.
8 12 "	3·21 E.	3·97 E.	4·72 E.	4·56 E.	5·30 E.	3·91 E.
10 12 "	1·04 E.	0·28 W.	0·19 E.	0·94 E.	0·59 W.	0·37 W.
0 12 P. M.	3·37 W.	3·80 W.	3·83 W.	3·86 W.	5·63 W.	4·84 W.
2 12 "	5·12 W.	4·60 W.	4·80 W.	5·31 W.	5·51 W.	4·52 W.
4 12 "	3·26 W.	2·45 W.	3·36 W.	3·41 W.	2·65 W.	1·51 W.
6 12 "	1·69 W.	0·48 W.	0·62 W.	1·19 W.	0·70 W.	0·54 W.
8 12 "	0·01 W.	0·64 E.	0·80 E.	1·01 W.	1·27 E.	1·22 E.
10 12 "	1·25 E.	0·45 E.	0·48 E.	1·03 E.	1·75 E.	0·54 E.

The following table shows the observations of the magnetic variation at Hudson. Column first shows the day of the month and year; column second the hour of observation; column third the observed variation; column fourth the correction applied to reduce the observation to the mean for the month; and column fifth shows the variation thus corrected. The correction is obtained by taking the mean of the numbers furnished by the Washington and Toronto observations.

Observations of the Magnetic Variation at Hudson.

Date.	Hour.	Variation observed.	Correction.	Variation corrected.
	h m			
1839 April 15.	3 30 P. M.	51·96 E.	+4·02	55·98 E.
1840 April 13.	9 40 A. M.	53·87	−1·88	51·99
1841 May 18.	6 51 P. M.	48·80	+0·18	48·98
1842 May 12.	2 43 P. M	49·23	+4·60	53·83
" Sept. 20.	10 57 A. M.	43·35	+2·76	46·11
1843 June 17.	4 23 P. M.	38·87	+3·04	41·91
" Sept. 19.	8 2 A. M.	47·65	−4·29	43 36
1844 June 8.	4 1 P. M.	38·20	+3·61	41·81
1849 July 24.	5 14 P. M.	30·04	+1·93	31·97
1858 July 22.	11 40 A. M.	9·37	+3·03	12·40
" July 26.	6 12 P. M.	11·47	+0·91	12·38
" July 28.	4 1 P. M.	5·61	+3·59	9·20
" Aug. 4.	0 6 P. M.	9·30	+5·73	15·03
" Aug. 9.	6 10 P. M.	10·58	+0·43	11·01
" Aug. 16.	11 37 A. M.	4·25	+4·52	8·77
" Aug. 17.	5 55 P. M.	9·50	+0 61	10·11
" Sept. 1.	2 32 P. M.	9·52	+4·04	13·56

In order to deduce from these observations the most probable result, let us put δ for the mean variation Jan. 1, 1839, and Δ for the annual motion. Then if we regard the observations of any one year as forming a single observation, we shall have the following eight equations of condition.

$$\begin{array}{llll} \delta + & 0{\cdot}285\,\Delta = & 55'{\cdot}98 & \delta + \quad 4{\cdot}586\,\Delta = \quad 42'{\cdot}63 \\ \delta + & 1{\cdot}279\,\Delta = & 51\,{\cdot}99 & \delta + \quad 5{\cdot}433\,\Delta = \quad 41\,{\cdot}81 \\ \delta + & 2{\cdot}375\,\Delta = & 48\,{\cdot}98 & \delta + 10{\cdot}558\,\Delta = \quad 31\,{\cdot}97 \\ \delta + & 3{\cdot}538\,\Delta = & 49\,{\cdot}97 & \delta + 19{\cdot}598\,\Delta = \quad 11\,{\cdot}56 \end{array}$$

Solving these equations by the method of least squares, we obtain $\delta = 55'{\cdot}213$, which is the mean value of the variation for Jan. 1, 1839; and $\Delta = -2'{\cdot}2416$; that is, the easterly variation is diminishing at the rate of $2\frac{1}{4}$ minutes in a year. We also conclude that the line of no variation will pass through Hudson sometime during the year 1863.

ART. XIX.—*On the Dynamics of Ocean Currents;* by Lieut. E. B. HUNT, Corps of Engineers, U. S. A.

(Read before the American Association, at the Baltimore Meeting, May, 1857.)

IT can scarcely be denied that the state of our knowledge of ocean currents is any thing but satisfactory. Not only are we to a very great extent ignorant of the precise state of the facts, but we are also deficient in the theoretical exposition of those already known. We can easily explain our lack of precise knowledge of facts by reference to the circumstances. The vast oceanic areas can be observed only by persons engaged in navigation, who are mostly unfurnished with proper means for correct determinations, and who lack that special training which is a prime

essential for good observations. The facts to be observed are also of a character so complex and elusive, are so subject to fluctuation in a given locality, and are involved in movements of air and water of so vast compass, that we cannot hope for precision of knowledge by the use of means now in operation. The single fact that most observations are made on the water surface while the ocean depths are of vital efficacy in shaping all marine phenomena, gives a character of signal incompleteness to those observations which have been mainly instrumental in fixing the received notions on the system of oceanic circulation. It is a fit subject of regret that the discussion of ocean movements has been so rarely attempted by those whose previous training in mathematical or mechanical science would have been a sufficient guaranty and preventive against the wild and illogical rhapsodies of theorists who have run riot over the broad domain of the physics of the sea.

With a view to apprehending the mechanical elements of this problem of ocean currents, let us first suppose a terrestrial sphere, which has assumed the equilibrium condition, resulting from gravitation, diurnal rotation, a solid nucleus and a homogeneous water envelope unbroken by land. This water stratum would shape itself so that its bounding surface would be a strictly mathematical level surface. A level surface of this nature may be defined as one which is at each point perpendicular to the resultant of all the forces acting on the individual molecules situated in that surface. In this case it would be a continuous oblate spheroid to which the resultant of gravity and centrifugal force would be everywhere normal. If to this we add those diurnal disturbances of the normal level due to the irregularities of solar and lunar attraction during the earth's rotation, we obtain the tidal waves which appear as perturbations of the normal level. If the continental masses be supposed to be elevated, we have a slightly modified normal level surface for the ocean; but one, which once determined becomes the proper standard of reference for all oceanic perturbations, to whatever cause due. This surface is everywhere the true bounding surface, and cuts the resultant of gravity and centrifugal action for the earth *as it is*, perpendicularly at each point of the surface, and is entirely continuous, though no more truly spheroidal. This is the normal ocean level, and it is a useful surface of reference for all vertical ocean movements or perturbations. If we now suppose the homogeneous earth without continents subjected to the heating action of the sun's rays, the result will be that the equator will become a line of maximum heating, from which to the poles there will be a progressive diminution of heat absorption. This would cause an expansion of the heated waters which would thus rise above the normal level surface by an amount equal to

the expansion at each point. Thus from the pole to the equator a spheroidal meniscus would be spread equal to the ocean expansion under the solar heat. It is a remarkable feature that the heating leaves each vertical ocean column of its original weight, and there is thus a perfect mechanical equilibrium between these columns considered as joined by their interior bases. Thus considering the grand ocean masses, there is no disturbance of static stability from the heating agency of the sun, hence there is no formation of massive currents due to this cause.

If now we regard the heated ocean in its hydrodynamic aspect, we find that the bounding surface having everywhere a slope toward the pole exceeding that of the normal level surface, there remains an unresisted surface tendency towards the pole which primarily tends to produce a superficial flow from the equator polewards. This gives manifestly but a slight disturbance of normal level, amounting in the meridian quadrant only to the vertical expansion at the equator, and being diffused over the entire quadrant. It is extremely doubtful if this would suffice to overcome the passive resistances and produce an actual surface overflow. If, however, a current were once established by any other agency, such as the wind, the equatorial heating would constantly operate to maintain this current. The heated waters would constantly be lifted as a floating mass on the colder waters which, pushing on the lighter equatorial mass at its base, would come in to replace any deficiency of mass due to the superficial outflow. Taking the facts as they are, we find in the trade-winds and the resistance of the continents, two causes fully adequate to break up the static equilibrium referred to, and obviously giving the precise direction to the outflow which it actually has. Thus while the type of action induced by the solar heating power considered along the meridian is surface outflow and deep inflow, the perennial trades determine this circulation along a different and constant route, fixed first by continental obstructions, and essentially modified in direction by the earth's rotation.

As the equatorial evaporation greatly exceeds the corresponding rain fall, this operates to counteract in part the regular outflow by diminishing the quantity of water to be discharged on account of expansion due to heating. This would also increase the saltness and specific gravity of the equatorial waters, and to that extent would bring their actual surface into close accordance with the normal level. It is clear that this saltness could not fully compensate for the expansion by heating, or we should have the surface reduced to the normal level, when all would either be in stable equilibrium, or below it when the currents would be reversed.

It thus appears that the expansion due to equatorial heats induces a superficial derangement tending to an outflow towards

the poles, which by the trade winds and continents is determined along a single line of *debouche*. This gives a discharge with far less frictional resistance than a direct meridional outflow would encounter, as this would involve a polar set for the entire ocean surface.

Accepting the well-determined trade-winds and the equatorial current as certain facts, we shall find that the vast surface sheet of water which has a westerly set under the trades, having acquired a very considerable velocity, becomes the representative of a vast amount of living force. When by impact against western barriers this vast sheet of water undergoes inflection to the north or south, it still retains the greater portion of its living force, and will continue to do so until this is wholly expended in overcoming resistances. If now we bear in mind that the wide equatorial sheet is by this deflection consolidated into a compact current of deep section, and also that the resistance per mile is proportionate to the length of the line of frictional resistance in a cross section, we shall see that the currents turn towards the poles with their forward impulse almost unabated, and with the resistances greatly reduced. We ought, therefore, to expect that the inertia of this vast moving mass would suffice to carry it on with a mean velocity, slowly abating, to the polar regions. So soon as the progress of the current gives it an increasing latitude, the effect of the diminishing parallels in giving an eastward trend would show itself; and, combined with the forward projectile motion of the mass of waters, would determine the route of the current, governed, of course, by solid opposing masses of continents, islands, and shoals.

Reaching the artic neighborhood, this current would fall in with the tendency to restore to the equatorial region the waters withdrawn by outflow, which thus leave a deficiency of static mass in that region. Its forward force not yet expended, would bring it into the equatorial flow only after a long arctic sweep. Then bordered in by the eastern ocean coasts, it circles on to the equatorial belt, there to start the repetition of its course either directly, or by proxy, if, entering at great depths, it serve only to lift higher portions above the normal level. We have thus a continuous circuit in which the water whirls under the primary impulse derived in the equatorial regions, an outflow due to heating and the direct propulsion of the trade-winds. The primary order of circulation is in two currents, the upper running polewards, and the under from the poles to the equator. This order is entirely modified by the action of the trades, and becomes essentially a horizontal circulation, the propelling action of these perennial winds, conspiring with the outflowing declivity to determine an immense movement, of which the living force imparted in the equatorial region suffices to carry on the circuit in full and enduring activity.

This consideration of the effect of inertia in storing the living force of this immense equatorial current, and thus enabling it to sweep through the cycles of the seas, has not been duly considered. These currents, in such a place as Florida Straits, move in a closed channel, and are subject to the hydrodynamic rules for this case. The gradual changes of direction and velocity there imposed, produce less absolute resistance than is generally imagined, by reason of the great mass of waters relative to the area of frictional surfaces.

The problem of ocean currents is of very great complexity, not only on account of the difficult hydrodynamic questions involved, but because the effect of the winds on the ocean surface can scarcely be subjected to estimation. The permanent elevation of the equatorial waters above the normal level traced from the pole, might be approximately determined by knowing the mean equatorial ocean temperature at all depths, and the same from point to point towards the poles, accompanied with observations on the corresponding saltness. Were there a considerable deficiency of weight in the vertical equatorial column, relative to an arctic one, connected by their bases at the same deep level, this would at once generate a corresponding wave towards the equator. As we may be sure that the equatorial mean ocean temperature exceeds the artic mean temperature, we must concede *some* elevation above the true normal level throughout all the warmer latitudes, but any attempt to definitely fix its amount, would be very rash in the present state of our knowledge.

There are numerous secondary points which might enter this discussion, but which need not now be considered. I will notice a slight oceanic oscillation which is practically unimportant; but which I believe has not before been noticed. The sun in its daily round must heat the waters of the sea, at a given locality, in such a manner that there shall be a daily maximum and minimum sea temperature due to absorption and radiation combined. This must give a maximum and minimum of expansion, or a species of tidal wave would follow the sun, which might well be called the heliothermal tide. It would clearly be too minute for separate observation, and though curious, cannot be important.

Another circumstance is worth notice here. A forward current in the sea has a distinct bounding surface on which it encounters a frictional resistance. The mode in which this resistance is expended is by a constant dragging into the forward movement parts of the layer of water making the boundary of the current. Thus if a current be moving through a sea otherwise tranquil, it will by this lateral dragging carry forward such a volume of water in addition to its own proper mass, that a counter-current must set in to restore the level. This is, I sup-

pose, the explanation of some of the counter currents which exist along the great oceanic currents, as also of the eddy currents of rivers. These too imperfect generalizations may do something towards making the system of ocean currents more comprehensible. So great a subject needs treatment far different from what it has yet received, and first of all the essential facts should be more clearly established. Unfortunately this can result only from long, well organized, and costly operations for this express purpose. We must be content to do our several small parts patiently, hoping for more light in the future.

ART. XX.—*Report on Dupont's Artesian Well at Louisville, Ky.;* by J. LAWRENCE SMITH, M.D., Prof. Chem. University of Louisville.

THIS work was commenced in April, 1857, from the bottom of a well that had a depth of 20 feet, the boring tools employed made a hole 5 inches in diameter to the depth of 76 feet from the surface; the boring was now reduced to 3 inches, and thus continued to the bottom of the well. The depth of well is 2086 feet; flow of water 330,000 gallons in 24 hours; rise above the surface 170 feet.

The rock struck, which geologically belongs to the Devonian series, is for 38 feet shell limestone; then for 40 feet coralline limestone; at which depth the Upper Silurian is reached. Without being able to make out with any degree of certainty, the amount of Upper Silurian passed through, we suppose it to be over 1200 feet. At the depth of 1600 feet a sandstone was reached, doubtless of the Lower Silurian, and 97 feet deeper was encountered the first stream of water which reached the surface. This flowed out abundantly and with much force. The quantity not being sufficient, the boring was continued. After this, it was unnecessary to use the bucket to take out the material detached by the borer, the force of the water bringing up the fragments very readily. The water increased in quantity in going deeper, the increase being more marked at 1879 feet, and still more at 1900 feet, where pieces of rock weighing an ounce or two came up with the water. The water increased every ten or twenty feet to the depth of 2036 feet; here a very hard magnesian limestone was encountered 6 feet in thickness. After which the sandstone reappeared, and for the next 50 feet there was no increase of water.

The following table exhibits the series of rock as far as it is possible to make it out by the fine fragments taken out at different depths, beginning at the top:

76 feet, sand and gravel.
100 feet, tolerably pure limestone, with fragments of fossils.
12 feet, soft limestone mixed with clay.
52 feet, tolerably pure limestone mixed with fossils.
5 feet, limestone with ferruginous clay.
81 feet, gray limestone.
157 feet, limestone mixed with clay.
149 feet, tolerably pure limestone with many portions quite white.
13 feet, clay shale with little calcareous matter.
207 feet, limestone with a little blue clay shale.
33 feet, same, little darker and more shale.
Next 94 feet, pure, very white limestone with fossil alternating with very dark limestone, color probably from organic matter, with some dark shale.
26 feet, shaly limestone.
40 feet, very light and hard pure limestone.
1 foot, white clay.
546 feet, gray limestone, alternating hard and soft.
41 feet, sand rock—white.
4 feet, same, very fine and hard, with little limestone.
60 feet, same, with more lime.
72 feet, same, less limestone.
308 feet, same sandstone with but little lime.
6 feet, magnesian limestone, very hard.
50 feet, sandstone again.

At the urgent request of many citizens of Louisville, the boring was now stopped to give a fair test of the medical virtues of the water that was pouring forth at the rate of 230 gallons per minute, or about 330,000 gallons in 24 hours. The water, by its own pressure, rises in pipes 170 feet above the surface.

The boring was accomplished in sixteen months, and the depth reached is 2,086 feet. In order to conduct the water to the surface and prevent its passing off into the gravel beds below, a tube five inches in diameter leads from the surface to the rock, a depth of seventy-six feet, into which it is driven with a collar of vulcanized gum elastic around it. No tubing is found necessary for any other part of the boring.

When the size of the bore (three inches in diameter) and its depth are considered, the flow of water from the well is unequalled by any other artesian well yet constructed that flows above the surface, for, although the Grenelle well at Paris delivers 600,000 gallons in twenty-four hours, it has at the bottom an area six times as great as the Dupont well, and a few hundred feet up seven times as great. A corresponding diameter to Dupont's well would, according to just and reasonable calculations, furnish about 2,000,000 gallons in twenty-four hours; also the elevation of the water above the surface is greater than that of any other artesian well, and it is only exceeded in depth by the St. Louis well, and that to an extent of 113 feet.

The water comes out with considerable force from the five-inch opening, and a heavy body thrown into the mouth of the well is rejected almost as readily as a piece of pine wood. By an approximate calculation, its mechanical force is equal to that of a steam engine with cylinder 10 by 18 inches, under 50 lbs. pressure, with a speed of 55 revolutions per minnte, a force rated at about 10 horse power. The top of the well is now closed, and the water conducted about 30 feet to a basin with a large jet d'eau on the centre, from which there is a central jet of water 40 feet in height, with a large water pipe, from which the water passes in the form of a sheaf. When the whole force of water is allowed to expend itself on the central jet, it is projected to the height of from 90 to 100 feet, settling down to a steady flow of a stream 60 feet high.

Temperature of the Water.—The water, as it flows from the top of the well has a constant temperature of 76½° F., and is not affected either by the heat of summer or the cold of winter. The temperature at the bottom of the well is several degrees higher than this, as ascertained by sinking a Walferdin's registering thermometer to the bottom, which indicated 82½° F. Taking as correct data that the point of constant temperature below the surface of Louisville is the same as at Paris, namely, 53° F., at 90 feet below the surface, we have an increase of 1° of temperature for every 67 feet below that point. The increase in Paris is 1° for every 61 $\frac{2}{10}$th feet. The temperature of the water is sufficient for comfortable bathing during most of the year, a circumstance that will be of considerable importance, if it ever be turned to the use of baths. The reason of the difference of 6° between the water at the bottom of the well and at the top is, that the iron pipe leading from the surface to the rock passes through a stratum of water 60 feet thick, having a temperature of 57°.

The Source of the Water.—The question naturally arises, if the vein of water supplying this well has a connection with some distant source higher than the surface of Louisville, where is that source? From all that we have been able to learn of the geology of this county, taking Louisville as a center, the first rocks encountered corresponding to the sandstone (in which the water of the artesian well was struck) are in Mercer, Jessamine, and Garrard counties, near Dix Creek, to the east of Harrodsburg. The rocks there are said to be cavernous and water-bearing. The elevation is about 500 feet greater than Louisville, and about 75 miles in a straight line from this city.

This being the most probable source of the water, from whence come its mineral constituents? These are obtained from the rocks through which it percolates in its way from its source to the point below Louisville, where it has been tapped, and where

it will doubtless flow in undiminished quantity for centuries to come, as wells having such deep sources as this, are usually inexhaustible.

Nature of the Water.—The water is perfectly limpid, with a temperature as already stated of 76½°, which will be invariable all the year round.

Its specific gravity is 1·0113. The solid contents left on evaporating one *wine gallon* to dryness are 915½ grains, furnishing on analysis:

	Grains.
Chlorid of Sodium, (common salt,)	621·5204
" Calcium,	65·7287
" Magnesium,	14·7757
" Potassium,	4·2216
" Aluminum,	1·2119
" Lithium,	0·1012
Sulphate of Soda,	72·2957
" Lime,	29·4342
" Magnesia,	77·3382
" Alumina,	1·8012
" Potash,	3·2248
Bicarbonate of Soda,	2·7264
" Lime,	5·9915
" Magnesia,	2·7558
" Iron,	0·3518
Phosphate of Soda,	1·5415
Iodid of Magnesium,	0·3547
Bromid of Magnesium,	0·4659
Silica,	0·8857
Organic Matter,	0·7082
Loss in analysis,	8·1231
	915·4582

GASES IN ONE GALLON.

Sulphuretted Hydrogen,	2·0050
Carbonic Acid,	6·1720
Nitrogen,	1·3580

The analysis was performed by the usual methods; but as chlorid of lithium was sought for and found, it may be of interest to detail the method of research in this particular, as a guide to similar investigations of other mineral waters in this country. Ten gallons of water were evaporated to about two pints, (there was an abundant deposition of salts,) to this was added one gallon of 95 per ct. alcohol; it was then thrown on a filter, and the salts on the filter washed with alcohol of the same strength—the filtered liquid was evaporated nearly to dryness; in the present instance the residue consisted of a few ounces of a thick syrupy liquid; to this was added one pint of absolute alcohol, addi-

tional salts were precipitated; the liquid was again filtered and evaporated nearly to dryness—to it were added 8 oz. distilled water and two ounces of milk of lime, (pure lime made by igniting carbonate of lime prepared by carbonate of ammonia,) the lime was added for the purpose of precipitating the magnesia and alumina—again filtered and washed; the filtered liquid was somewhat concentrated, and while warm, carbonate of ammonia added to precipitate the lime; it was then filtered and evaporated to about a fluid ounce and treated with a little lime water and carbonate of ammonia alternately, to insure the absence of the last traces of magnesia and lime.

Before going further, it would be well to state that the treatment of alcohol separates the great mass of salts that are held in solution by the water, and which interfere with the detection of so minute a constituent as the lithium salt—by the alcohol we reduce the salts to small amounts of chlorids of magnesium, aluminum, calcium, sodium, potassium and lithium; by the lime the first two are got rid of, and by the carbonate of ammonia the lime is precipitated.

The solution, now containing the chlorids of sodium, potassium, lithium and ammonium, is evaporated to dryness, and the residue heated to dull redness, by which the ammonia salt is expelled and a little organic matter destroyed; the residue is next dissolved in water, and a drop or two of the liquid tested for a sulphate; should this be present it must be got rid of by exact neutralization with chlorid of barium, (a slight excess of the chlorid of barium will not interfere with the other steps in the analysis); in the examination of the water in question no trace of sulphate was found at this stage of the process, so it was again evaporated to dryness in a small capsule over a water bath; there were now a few grains of residual matter. To this was added an ounce of a mixture of equal parts of pure ether and absolute alcohol, the capsule was covered with a small receiver and allowed to stand for 18 hours; the liquid was then thrown on a small filter, and the filter washed with a little of the mixture of ether and alcohol. The alcoholic ether solution evaporated to dryness furnished the chlorid of lithium recognized by its well known characteristics. Although this process requires considerable time and some careful manipulation, its results are both accurate and satisfactory.

The water of this artesian well has very valuable medical properties, and those readers who are curious to examine into these points, will obtain all the required information by sending to Louisville for the medical report.

ART. XXI.—*On Modes of increasing the Heat of the Mouth Blowpipe, and some new Blowpipe Manipulations;* by Prof. HENRY WURTZ, of the National Medical College, Washington, D. C.

[Read, with experimental illustrations, before the American Association for the Advancement of Science, at Baltimore, April, 1858.]

IN the course of some blowpipe investigations which I have in progress, it has been found extremely difficult, and sometimes impossible, to obtain in the ordinary way sufficient heat for the production of certain desired effects. Attempts were therefore made to devise means of increasing the heating power of the instrument, and this object has been so far attained, and by means so simple and efficient, that I take this opportunity to make these means public, that others may also be benefitted thereby. To blowpipe analysts it is not necessary for me to detail the advantages to be gained in many cases by a practicable mode of increasing the heat, of which advantages not the least important is the saving of time.

It was first observed that in the ordinary mode of manipulation, a great part of the heat was conducted away from the bead by the cold part of the platinum wire contiguous to it. This is easily prevented by simply bending the wire previously, at right angles about an inch or an inch and a half from the loop that is to hold the bead.* On then holding the bead at the point of the blue cone of the flame, and the wire so that the bent portion is coincident with a continuation of the axial line of the flame, this bent portion becomes also heated to high redness, losing thus in a great degree its tendency to abstract heat from the bead. By this little contrivance alone the heat is increased to so important a degree, that I venture to think that no one who has once tried it, will ever use a wire of any other form.

I next directed my attention to the *combustible* used. An ordinary alcohol flame, as every one knows, gives with the blowpipe a comparatively feeble heat. A gas flame is much superior, and a large *wax candle* gives probably a higher heat than any thing else at present commonly in use among blowpipe operators. It occurred to me that the heating effect was probably proportional to the *density* of the burning vapor, or the quantity of

* At the time the above was read, I had no suspicion that the contrivance of bending the wire had ever been published before, or thought of, by any other than myself. Since, however, Prof. Brush of New Haven has directed my attention to a passage in the last edition (1853) of Plattner's "Probirkunst mit dem Löthrohre," page 14, where the same device is identically described, as used for fusing platinum wire. I desire, therefore, stating at the same time that I have been in the habit of using it for more than ten years, to disavow all claim to priority, hoping still that some novelty may be found in my modes of making use of the invention.—H. W.

combustible matter contained in the same volume. I therefore searched for combustibles having a high density of vapor, and found that the *paraffine* of Reichenbach, now known from the investigations of Hofstaedter* and Filipuzzi† to be a mixture of different isomeric hydrocarbons, all of which must have very high equivalents, was found by Lewy‡ to have a vapor-density of not less than 11·8. By inquiry, I found that candles composed of this, or a similar material, obtained from the products of distillation of the well-known "Breckenridge Coal," could be bought in New York. On procuring some of these, and using them as *pabulum* for the blowpipe-jet, I found my anticipations fully realized. The flame obtained by means of the paraffine candle is much hotter than that from a wax candle. Unfortunately the candles made in New York are small, and have *extremely* small wicks, which renders them difficult to manipulate with, because the least motion of the jet-piece, by throwing it out of the centre of the flame, deranges the form of the cone. This can of course be easily remedied by having larger candles made with larger wicks.

Next, and lastly, *the blowpipe itself* seemed to me susceptible of improvement, by the introduction of an agent to absorb the moisture and carbonic acid of the breath, which must necessarily diminish the heating power of the flame. I have therefore sought to eliminate these obstacles by using, instead of the tube of the ordinary blowpipe, a somewhat larger tube filled with fragments of caustic potash. In the instrument which I have heretofore used, and which I now exhibit, this tube is composed of glass, and is united with the jet-piece of an ordinary blowpipe of Berzelius' form by means of a perforated cork; but of course in practice this tube may as well be composed of metal to avoid breakage. I use the ordinary *potassa fusa*, occurring in the shops in the form of sticks, broken up to about the size of a split pea, the fragments being confined in the tube by a plug of cotton at each end. It is advisable, when this kind of blowpipe is not in use, to keep the upper end of the tube corked. It remains yet to be seen whether the additional advantage gained by using a blowpipe of this construction will compensate for the concomitant inconveniences, though the latter are far less important, according to my own experience, than many would suppose.

Now as to the effects which may be produced by the combination of these several appliances. Platinum wire of medium blowpipe size is fused with little difficulty, and I have obtained beads of considerable diameter. Fine platinum wire melts down

* Liebig and Kopp's Jahresbericht, 1854, 608.
† Ibid. 1855, 630.
‡ Loewig's Chemie der organischen Verbindungen, ii. 564.

like wax. A borax bead volatilizes rapidly in white smoke. By moistening the end of a bent platinum wire, and dipping it into pure carbonate of lime or pure magnesia, and exposing it to the flame, a very fair exhibition of Hare's lime light, in miniature, may be made. The fusion of a small iron wire, and the combustion with brilliant scintillations of a small steel watch spring may be exhibited as class experiments in a very striking manner.

Berzelius, in his work on the Blowpipe,* says, "I am convinced that the temperature produced by the blowpipe, fed by air from the lungs, has a distinctly defined limit; so that, for instance, *alumina* or *silica* cannot be melted, however small fragments of them be employed." Now although I have not yet succeeded in producing large globules of fused silica, yet any person, by adopting the above expedients, may convince himself that the degree of heat thus obtainable is adequate for the fusion of silica. My experiments were made with chemically pure precipitated silica, and with a fragment of a colorless transparent crystal of quartz, from Herkimer county, New York, finely pulverized in an agate mortar. By taking a small platinum wire, bent as above, first fusing the end into a globule, then moistening this globule with saliva, or better (in order to avoid the introduction of any trace of *basic* contamination, which might be supposed to form a fusible silicate), with *syrup* made from pure sugar, and dipping it into the powder, then gently heating and incinerating in the flame of a spirit lamp, the silica powder remains loosely attached to the wire, and under the lens appears now perfectly impalpable and devoid of transparency. If now the potash-tube blowpipe, with paraffine candle flame, be brought to bear upon it for a minute, the silica being held a little way *outside* of the point of the blue cone, which in this kind of flame appears to be the hottest point, it will then be found strongly adherent to the wire, as if melted fast, and under the lens presents the appearance of small, transparent, irregularly shaped globules, *fused fast* to the platinum. I have little doubt that with a large paraffine candle, and larger jet, a *splinter of quartz* might be fused.†

The above mode of obtaining a high heat is essential to the practicability of a peculiar mode of manipulation that I have devised, and which I shall proceed to describe.

* Whitney's Translation, page 46.

† Berzelius, in a note, mentions an announcement of H. de Saussure that he melted *quartz*, supported on a slip of *kyanite*, with a jet of air from a *double bellows* through the flame of a thick *wax candle;* adding, however, that he suspects that "the support may have produced an effect on the assay, and that the air from the bellows, being purer than that of the lungs, may have also contributed to effect a result which cannot be obtained by the mouth blowpipe."—*Whitney's Translation*, p. 54.

Many minerals and artificial products, when dissolved in a borax bead *in large quantity*, cause the bead to become opaque and milky on cooling, forming in fact an *enamel*, instead of a glass. Some of these are *lime*, *magnesia*, *baryta*, *strontia*, *glucina*, *oxyds of zinc*, *cadmium* and *cerium*, *tungstic* and *titanic acids*, *calcite*, *magnesite*, *dolomite*, *witherite*, *baryto-calcite*, *strontianite*, *gypsum*, *epsomite*, *heavy spar*, *celestine*, *goslarite*, *smithsonite*, *fluor spar*, *apatite*, *sphene*, *aeschynite*, *polymignyte*, *yttro-tantalite*, *scheelite*, *scheeletine*, *xenotime*, *yttro-cerite*, *fluocerite*, *cryolite*, &c. A great many more render the bead of *microcosmic salt* opaque or opalescent on cooling, and as some reactions, for instance that of *titanic acid*, are much more easily obtained in this than in the borax bead, these cases are also frequently of importance.

Now when it is desired to detect another substance occurring in small quantity in any of the above, such for instance as *manganese*, *copper*, *cobalt*, or *titanium*, by the color imparted to the bead after cooling, it is frequently impossible to do so, for the reason that if enough is added to give a decided reaction, the bead becomes opaque on cooling and the color cannot be seen.* In considering the cause of this loss of transparency, it seemed to me that it must be a granular *crystallization*, and remembering the fact that even *common glass*, if made to cool very slowly from fusion, becomes opaque and enamel-like, with many other familiar facts of an allied nature, it occurred to me that the converse of this phenomenon might also take place, or that an enamel which becomes transparent and vitreous when fused might retain its transparency when *suddenly* cooled. I was thus led to dip a bead composed of a highly basic borate of lime enamel into cold water while still fused. The result verified the hypothesis. The bead when cooled in this way remained transparent, and manganese was thus distinctly detected in a purely white marble, in which its presence could be distinctly pronounced upon otherwise only by Crum's test.

For a number of years I have had constant occasion to use this expedient, and have found it a highly valuable one. Blowpipe operators will need no further remarks upon the importance of such an addition to our facilities for investigation. It is necessary, however, that careful examination be made of all special cases, a work of time and patience, which I hope, nevertheless, soon to take up systematically.

As before intimated, it is necessary, or at least highly conducive, to the success of this new manipulation, to have means, such as are above detailed, of obtaining an increased heat; be-

* Speaking of *red zinc ore*, Berzelius says that it is dissolved by phosphorus salt, but that "the color of oxyd of manganese cannot be obtained till the glass has dissolved so large a quantity that it is no longer transparent on cooling."—*Whitney's Translation*, page 116.

cause these supersaturated beads, which *enamelize* (if I may be allowed to coin a new but convenient word) on spontaneous cooling, require a much higher temperature for fusion than those which remain vitreous, and without some additional facilities for obtaining this higher heat, the tax upon the time and patience of the analyst becomes very onerous, and in many cases I have found it in fact *impossible* to succeed by the ordinary mode, for when the bead is very highly basic, it must be heated considerably *above* its point of fusion, or it solidifies to an enamel immediately on removal from the flame. It was this necessity which led me to devise the above modes of increasing the heat. It is by no means necessary, however, in ordinary cases, to use *all* of the expedients described. In fact, for the fusion of most of these borax enamels, the *bent wire* alone, with an ordinary blowpipe and an alcohol or gas flame, will suffice, and most others need only the additional assistance of the paraffine candle.

Whilst upon this subject of the blowpipe, I may be pardoned for offering briefly another suggestion. While making the experiments upon the reciprocal neutralization of the colors of metallic solutions, described in a former paper,* it struck me that the same principle should have applications in blowpipe analysis. It *must be* that when several metallic oxyds are present in a glass their several colors must interfere with each other in certain ways, and if we knew how to eliminate one or more of these colors by adding a neutralizing ingredient, so as to render others apparent, might it not furnish us with new facilities for research, and enable us to avoid old sources of error? I may conclude by noticing two or three results, which I have obtained, bearing upon this question. A deep amethystine bead of manganese glass acquired, on addition of a little oxyd of chrome, a *gray* color, resembling that produced by a mixture of solutions of chlorid of cobalt and chlorid of chromium, without any tinge of either red or green; a trifling additional quantity of oxyd of chrome giving, however, a distinct green color.† Another manganese bead gave a similar gray color on addition of oxyd of copper, but a slight excess of the latter imparted then a *blueish* tinge, which might easily be attributed to a trace of *cobalt*. The presence of nickel does not affect the blue color of a cobalt glass, as it does the rose color of cobalt in aqueous solutions, unless its quantity, compared with that of cobalt, is very great.

* Am. J. Science [2], xxvi, 49.

† In the decoloration of *ferriferous glass* by the addition of deutoxyd of manganese, "glass-makers' soap," is the action *wholly* due to the conversion of the ferrous into ferric oxyd, or *partly* also to the neutralization of a portion of the green ferrous color by some violet manganese glass formed?

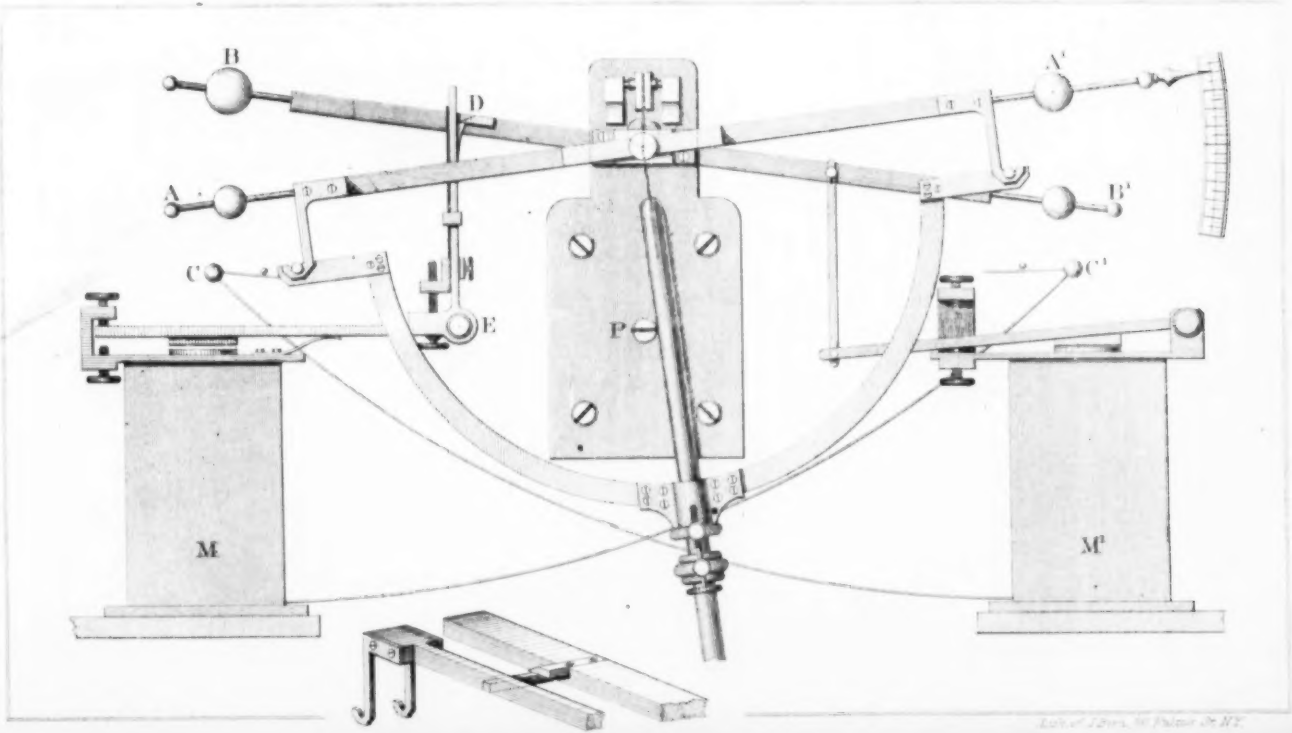
ART. XXII.—*On the Pendulum;* by F. A. P. BARNARD, President of the University of Mississippi,—*with a description of an Electric Clock,* constructed by E. S. RITCHIE, of Boston, for the University of Mississippi, under the direction of President Barnard.—With a Plate.

[From the Proceedings of the American Association at Baltimore, 1858.]

THE importance of the pendulum as an instrument for the measurement of time, is sufficient to justify any amount of effort which may be made to secure the regularity of its performance. The causes which disturb this regularity exist partly in the nature of things, and are partly introduced by the contrivances employed to maintain the motion of the instrument. Among these, the effect of varying temperature in altering the distance between the centre of oscillation and the point of suspension, is one which has given occasion to many ingenious inventions; yet, however effectual some of these may have been in removing the irregularity due to this cause, it is probably true that no plan of compensation has been found in practice to be entirely satisfactory. It is an opinion entertained by the writer, though it is proposed with some diffidence, that the problem of compensation cannot be experimentally studied with results to be perfectly relied upon, so long as the pendulum has any *work to do;* and this must be the case whenever the maintaining power is derived, directly or indirectly, from a train of wheel-work. The different forms of anchor or pallet escapement involve friction upon the pallets, which, however nearly constant it may be, cannot be wholly so, and however slight it may be, either absolutely or in its variations, cannot be altogether insensible as a disturbing cause. For small as may be the amount of fluctuation in this resistance, it is to be considered that all the quantities concerned in the question of maintaining the motion of the pendulum are small, and that every minute variation is multiplied thousands of times. But a more serious cause of irregularity in the pendulum directly driven by a train, is to be looked for in the varying condition of the train itself, and of its lubricants; in consequence of which the power of the prime mover is to some extent absorbed, and is at different times unequally communicated to the pendulum.

When we attempt to study, in the actual performance of the pendulum, the efficacy of any plan of compensating the effects of temperature, it is impossible entirely to distinguish the irregularities due to one cause from those which may proceed from another. Cold, for instance, by stiffening the lubricants may cause the clock to gain, and this effect may be erroneously ascribed to an under-compensation of the contraction of the

DIAGRAM OF AN ELECTRICAL CLOCK DESIGNED BY PROF. F. A. P. BARNARD



pendulum-rod. And though, in such a case, if the clock should run more slowly than in warmer weather, we might justly infer an over-compensation to exist, we should be unable to determine exactly the excess.

The partial or total failure in practice of plans of compensation theoretically perfect, has been sometimes attributed to the unequal rapidity with which changes of temperature take place in different parts of the same pendulum. Every plan of compensation is necessarily founded on the supposition that, under all alterations of temperature, the temperature throughout the entire instrument is simultaneously the same. It is easy to see, for example, that if the mercury employed to compensate the pendulum of a common astronomical clock were to be wholly inclosed in a cylinder of some material entirely impervious to heat (supposing such a material to exist), it would be altogether useless for the purpose intended. And that which would thus be true, on the supposition that the mercury could not change its temperature at all, must be measurably so, if its changes of temperature lag behind those of the rod. Glass jars for containing the mercury in pendulums of this construction have been objected to, on the ground that they do introduce an irregularity of this sort; and accordingly Mr. Dent, the distinguished practical horologist of London, introduced in place of them cylinders of iron. The objection has been founded, I believe, rather upon the observed performance of the pendulums, than upon actual observation of the relative temperatures of the mercury and the rod. It would seem not to be difficult to arrange a pendulum with thermometers which should show constantly the true temperature both of the mercury and of the rod. And considering the importance of the question involved, such an experiment would appear to be well worth making, before pronouncing the mercurial compensation to be unsatisfactory, or even condemning the glass cylinders.

The escapements called remontoirs apparently set the pendulum free from most of the liabilities to disturbance which the train introduces. In clocks provided with these escapements, the force of the train itself is exerted not in impelling the pendulum, but in raising a small weight, or bending a slight spring, which subsequently acts—the former in its descent or the latter in its recoil—in moving the pendulum. The gravity-remontoir apparently furnishes an impelling force which is perfectly uniform, there being nothing but the very slight friction on the pivots of the arms carrying the remontoir weights to disturb this uniformity. The spring-remontoir is free from even this source of disturbance, but is open to a more serious objection, in consequence of the varying elasticity of the spring occasioned by change of temperature. Both of these contrivances, however,

impose a certain duty on the pendulum, which is, to unlock the train at the moments when it is necessary that the remontoir motor should be raised. This cannot be done without friction, although the friction is less than in the case of pallet escapements. It is also true that, in proportion as the friction required to unlock is reduced, these escapements become liable to the accident of failing to lock—or of *tripping*, as it is technically called—whereby error is introduced into the time shown upon the dial.

Of all these escapements, remontoir escapements and pallet escapements alike, it may finally be said that they require the pendulum to swing through a certain arc larger than would be necessary if the maintaining power could be applied to it from without, leaving it subject to no disturbances whatever, beyond those occasioned by the varying temperature and density of the air. The usual extent of the arc of vibration is 2° on each side of the vertical. About one quarter of this distance is required merely to unlock the train of the remontoir. If now, at a definite point of the swing, a light weight could be deposited upon an arm projecting from the pendulum rod, allowed to remain there during the descent, and then removed, and if this could be repeated on one side and on the other alternately, with perfect regularity and at precisely the same distance always from the centre of motion, and if this could be done without any friction or concussion, we should have a pendulum subject to no forces accelerating or retarding but such as may be accurately estimated in their amount, and in their effects upon the time of vibration.

I do not overlook the fact, that the manner of suspending the pendulum may have some influence on its performance. But as the suspension is almost invariably by means of a very flexible but also very elastic spring, the effect due to the resistance of this spring in the ascent may be considered as neutralized, so far as regularity of vibration is concerned, by its recoil in the descent; and for the variations of its elasticity with change of temperature, a special compensation may be made.

The problem which is here proposed, seems to present a condition difficult to be fulfilled. The impulse is to come at the proper moment, but the pendulum is to do no work in order to induce it. Yet it can be nothing but the pendulum itself which is to determine the moment of application; since if we possessed any independent mechanism sufficiently regular to do this, we should have no need of the pendulum. It is believed that the difficulty which this consideration presents has been overcome.

The clock which is herewith presented for the examination of the Association is one in which electricity is made to work a remontoir apparatus, by which very slight weights are made to impel the pendulum, alternately, on either side. There is nothing new in the idea of an electric clock; but there is something

sufficiently novel in a clock in which the pendulum does absolutely no work at all (not even in making battery connections), to deserve attention. There have probably been as many varieties of electric clocks as of escapement clocks—conceived, at least, if not constructed. All of these known to the writer, involve as much friction as the dead-beat escapement—most of them a great deal more—or else are otherwise objectionable. That which seems to be least so, is a contrivance described in the third volume of Becquerel's Electricity, and attributed to Mr. Vérité, in which two light balls are suspended by metallic threads to a horizontal lever oscillating on pivots placed just above the point of suspension of the pendulum. Two arms from the pendulum alternately touch these balls, closing a battery circuit by the contact. The corresponding end of the oscillating lever is thereupon depressed, relaxing the suspending thread, so that the ball presses upon the pendulum arm until the latter is carried by the swing out of its reach; when, the circuit being broken, the lever rights itself again. Ingenious as this arrangement is, the objections to it are too obvious to require enumeration. It furnishes a force which fails in the three essential requisites,—perfect uniformity in quantity, uniformity in duration, and uniformity in the point of application. The suspending thread, however flexible, must interfere with the first of these conditions, and the agitation produced by the sudden tilting of the lever must affect the other two.

The construction of the clock herewith exhibited, may be explained by reference to the accompanying diagram, (see Plate,) which shows the upper portion of the pendulum rod with the contrivance employed to apply the impelling weight. Two levers are represented, marked $A\ A'$ and $B\ B'$.

The first of these, from its office, is called the remontoir lever —the second, which is employed to control the first, is called the governor. The former is provided at each of its extremities with two hooks (as shown in perspective at the bottom of the diagram). These hooks are designed to carry a very light weight in the form of a small cylinder of metal. This cylinder is slightly indented in the latter at the points where it rests on the hooks, to prevent liability to displacement.

The pendulum-rod is furnished with two semicircular arms, to which, at their extremities, are attached two parallel plates of metal which pass between the hooks of the remontoir lever, and receive the weights, at the proper moments from the hooks, being also slightly notched or indented, to secure uniformity in regard to the point of application of the force.

Both the levers are pivoted in agates, their pivots being in the same horizontal line as the centre of motion of the pendulum. They are also provided with adjustable weights at their extremi-

ties designed to make them tilt slowly in one direction or the other, as may be necessary in order to apply the impulse on either side of the pendulum at the proper moment.

The remontoir lever, when free from control by the other, tends to preponderate toward the right. The governor has a sufficient preponderance in the opposite direction to carry the remontoir with it, when in action, by means of a projection seen at *D*, which overlaps a corresponding projection on the other, as shown in perspective at the foot of the diagram. This lever in the position exhibited, is caught by a detent at *D*, and the remontoir lever is free to tilt toward the right, in doing which, it will carry the impulse weight of the extremity *A* along with it, while it will leave that at the extremity *A'* on the higher arm of the pendulum. In its swing toward the left, the pendulum will then be impelled by this small weight until it reaches a corresponding inclination on that side, when it will deposit the weight *A'* upon the hooks of the remontoir, and will take up the other upon the opposite arm.

This mechanism is so simple as to require no further explanation. It only remains to point out in what manner the levers are controlled by the electric battery.

The remontoir lever is insulated by the agates in which it turns. By a tangent spring at its axis it is put into the battery circuit. It has no other contact with the mechanism except where it is acted on by the governor; at which point insulation is also effected; and likewise through the springs shown at *C* and *C'*, upon which the hooks remotest from the observer alternately rest. These springs are fixed in insulated pins to which are soldered the extremities of the enveloping wires of the magnets *M* and *M'*. The pin *C* is connected with the magnet *M'*, and the pin *C'* with the magnet M. The continuation of these wires beyond the magnets is not shown; but they are united into a single one, which, after enveloping a third magnet in the time-register (of which no drawing is given), returns to the battery.

From what has thus far been said, it would appear that the moment the hook of the remontoir lever touches either of the springs, *C* or *C'*, the battery circuit would be complete. But this is not the case: for the hook in contact with the spring is insulated from the lever, and the circuit is only completed at the moment when the pendulum, in its swing, deposits the impulse weight upon the two hooks.

In the position shown, the weight *A* is supposed to have just been deposited. The magnet *M'* has acted and has raised the extremity *B* of the governor to the detent, *D*. The remontoir lever will now slowly tilt, the gentle motion being necessary to prevent the impulse weight from being thrown off; and the balance weights being so adjusted as to secure the necessary

change of position within the second. A stop prevents the spring *C* from following the hook as it rises.

When the impulse weight *A'* is deposited on the hooks, it is the magnet *M* which acts; and the effects of this, through the bent lever pivoted at *E*, is to release the governor, which, by its preponderant weight will cause the remontoir to tilt again.

Whenever either magnet acts, the magnet of the time-register simultaneously acts, and advances the second-hand one division on the dial.

The remaining parts of the mechanism it is hardly necessary to describe. Adjusting screws are provided to secure the exact position of the pendulum arms, and to cause the impulse to be precisely equal in duration on opposite sides. In adjusting for this latter purpose, the graduated arc on the right, and the index attached to the remontoir lever, are employed. The manner of making the adjustment is obvious.

A pendulum impelled in this manner is subject to the action of no forces which cannot be definitely appreciated. The impelling power is constant and known. The mean resistance of the air may be computed, and even its fluctuations may, if necessary, be taken into account. The irregularities, therefore, which cannot be ascribed to these causes, must be due to imperfect compensation.

There is a possibility that a steel pendulum rod may, to some extent, be affected by the vicinity of the electro-magnets employed in this contrivance. In order to guard against this danger, if it be one, the rod of the pendulum of the clock, here exhibited, is made of brass; and the compensation, which is mercurial, is adjusted accordingly.

The effect of the impelling and resisting forces acting upon a pendulum is to alter its rate of motion; but this circumstance is of no importance, so long as these forces are invariable, like gravity. If, however, any variation occurs, either in the impulse or in the resistance, the time of vibration will be altered. The kind of alteration which occurs, in consequence of a given change of arc, is not, nevertheless, the same, with pendulums impelled on different plans. The recoiling escapement, for instance, accelerates the rate for an increase of arc, while the dead beat retards it beyond the amount which would be due to circular motion, as compared with motion in the cycloid.

The remontoir escapement has the advantage that, though it accelerates the rate of going of the pendulum, it applies invariably the same amount of impelling force at every swing; so that if the pendulum had no work to do in the unlocking of the train, it would be subject to no disturbance of its regularity except such as may be consequent upon fluctuations of atmospheric

density, and upon changes in its own temperature. In studying experimentally the subject of compensation, it would not be difficult to eliminate the effects of the first of these causes, so as to exhibit truly the merit or defect of any given mode of compensation for the expansion and contraction of the materials of the pendulum.

The remontoir escapement does not perfectly fulfil these conditions; but it is believed that the electric clock herewith presented does so completely.

This pendulum has an additional advantage over an escapement remontoir; which arises from the fact that its arc of vibration may be reduced much lower than is at all practicable with a clock driven by a train. All the errors of the pendulum, except those arising from the varying temperature of the rod, increase with the arc. It is believed that this pendulum may be run with so small a motion as to make such errors quite insensible. The degree, moreover, to which external forces affect the rate without altering the arc, is proportional to the forces themselves; and these, in the present case, must necessarily be less as the arc is less.

In order to show in what manner a pendulum of this description differs in its rate of going from one entirely free and vibrating in vacuo, we may take the ordinary differential equation of the angular motion of an oscillating body, and introduce into it terms expressing the forces which in this case are in action, besides gravity. This equation then becomes (φ being the variable arc, measured from the vertical, t the time of one vibration, g the force of gravity, l the length of the simple pendulum, m the maintaining and r the resisting forces, or rather their constant coefficients as compared with gravity).

$$\frac{d^2\varphi}{dt^2} = -\frac{g}{l}\left(\sin\varphi - rf(\varphi) + mf'(\varphi)\right).$$

The maintaining force, in the present case, is a weight applied at the extremity of an arm of the pendulum at the height of the centre of motion. Represent the weight by w, the length of the arm by a, and the total mass of the pendulum by M, and we have the value of m equal to $\frac{wa}{Ml}$. The function of φ on which its effect depends is obviously the cosine.

The resistance is, in this case, nothing but the atmospheric inertia, so long as the impulse lasts; after this, the maintaining weight becomes itself a resisting force, and its sign must be changed. The resistance of the atmosphere may be computed on the supposition that the velocity with which a falling body, of equal weight with the pendulum, and presenting an equal surface of resistance, ceases to be accelerated by gravity, is

known. This velocity may be represented by k. Then $rf(\varphi) = \frac{v^2}{k^2}$. For the value of $f(\varphi)$, we consider the velocity to be that which a body would acquire in falling vertically through the height the pendulum has descended from the commencement of its motion, which (if α represent the limit of vibration) is

$$l(\cos\varphi - \cos\alpha).\quad \text{Then } v^2 = 2gl(\cos\varphi - \cos\alpha), \text{ and}$$

$$rf(\varphi) = \frac{2gl}{k^2}(\cos\varphi - \cos\alpha).$$

The equation then becomes

$$\frac{d^2\varphi}{dt^2} = -\frac{g}{l}\left(\sin\varphi - \frac{2gl}{k^2}(\cos\varphi - \cos\alpha) + \frac{w\,a}{Ml}\cos\varphi\right).$$

If we suppose the impulse to continue to the distance β on the other side of zero, and integrate between the limits α and $-\beta$, we shall have (employing the simpler symbols for the sake of convenience)

$$\frac{d\varphi^2}{dt^2} = 2\frac{g}{l}\left[\cos\varphi - \cos\alpha + (m-r)(\sin\alpha - \sin\varphi) + r\cos\alpha(\alpha - \varphi)\right].$$

Replacing the trigonometrical functions by their values in terms of the arcs, and rejecting minute terms of higher orders than the second, we shall obtain, after reduction,

$$\frac{d\varphi^2}{dt^2} = \frac{g}{l}\left[\alpha^2 + 2m\alpha - 2m\varphi - \varphi^2\right].$$

Hence,

$$dt = \sqrt{\frac{l}{g}}\frac{-d\varphi}{\sqrt{\alpha^2 + 2m\alpha - 2m\varphi - \varphi^2}},$$

$$t = \sqrt{\frac{l}{g}} \times -\arcsin\frac{\varphi + m}{\alpha + m} + C.$$

And, between the limits α and $-\beta$,

$$t = \sqrt{\frac{l}{g}}\left(\frac{\pi}{2} - \arcsin\frac{-\beta + m}{\alpha + m}\right).$$

In like manner, if t' express the time during which the impulse weights oppose the motion,

$$t' = \sqrt{\frac{l}{g}}\left(\frac{\pi}{2} - \arcsin\frac{\beta + m}{\alpha + m}\right).$$

Putting $\tau = t + t'$, the total time of vibration,

$$\tau = \sqrt{\frac{l}{g}}\left(\pi + \arcsin\frac{\beta - m}{\alpha + m} - \arcsin\frac{\beta + m}{\alpha + m}\right).$$

Developing these arcs in terms of their sines, according to the ordinary series, taking their difference, and expressing by Δt the difference between t and the time of vibration of a free pendulum,

$$\Delta t = -\sqrt{\frac{l}{g}}\left[\frac{(\beta+m)-(\beta-m)}{(\alpha+m)} + \frac{1}{2.3}\frac{[(\beta+m)^3-(\beta-m)^3]}{(\alpha+m)^3} + \frac{1.3}{2.4.5}\frac{[(\beta+m)^5-(\beta-m)^5]}{(\alpha+m)^5}, \text{ &c.}\right];$$

which becomes, if we neglect insignificant terms,

$$\Delta t = -\frac{2m}{\alpha+m}\sqrt{\frac{l}{g}}\left[1+\frac{\beta^2}{2.\alpha^2}+\frac{\beta^4}{2.4\alpha^4}+\frac{\beta^6}{2.4.6\,\alpha^6}, \text{ &c.}\right] \quad (1)$$

If $\sqrt{\frac{l}{g}}=\frac{1}{\pi}$, as for the free pendulum beating seconds, then, putting ΔT for the total acceleration of the clock in making the number of beats made by the seconds pendulum in a day, and calling the entire remaining value of the second member of the last equation S,

$$\Delta T = \frac{86400\,S}{\pi}. \quad (2)$$

The terms containing r having disappeared from these expressions, it would seem that the resistance of the air does not affect the time of vibration. These terms, however, have not been eliminated, but only neglected, in consequence of being connected with powers of the arc higher than the square. By preserving them, it may be shown that the resistance of the air produces an effect which is not altogether insensible; rather, however, by consuming some of the disturbing power of m, than by its direct influence. The reason of this is, that the resistance of the air opposes gravity during the descent of the pendulum, but favors it during the ascent.

The value of the foregoing series depends upon the impelling power, and on the ratio between the arc of impulse and the arc of vibration. The necessary impelling force itself, when the ratio just mentioned is fixed, depends upon the absolute magnitude of the arc α. If we assume this arc at two degrees, which is ·035 of the radius taken as unity, and make the ratio of β to $\alpha = ·7071$, as recommended by Mr. Denison, in his rudimentary treatise on clock and watch work, we may compute the value of m by the following process:—

Assume the pendulum to weigh twenty pounds, which is not far from the weight of that of the clock exhibited; and suppose it to expose a resisting surface to the air of thirty square inches. A column of air of equal base and weight would be about 1250 feet in height, and the velocity with which a fluid of this altitude

would issue from a vessel in consequence of the superincumbent weight, is determined by the formula

$$v^2 = 2gh.$$

Were the pendulum therefore to move with a velocity equal to the square root of $2 \times 32 \times 1250$ ($= 80{,}000$), the resistance would be equal to its weight. Hence,

$$k = \sqrt{80{,}000.}*$$

And $r\ (\cos\varphi - \cos\alpha) = \frac{2gl}{k^2} \times \frac{\alpha^2 - \varphi^2}{2} = \frac{gl(\alpha^2 - \varphi^2)}{80{,}000}$.

This being the coefficient of the variable resistance, the total effect of the resisting force may be found by integrating the expression,

$$\frac{gl(\alpha^2 - \varphi)^2}{80{,}000} g\,dt,$$

between the limits α and $-\alpha$. Employing the symbol r, instead of the fractional coefficient, and taking the value of dt as given above, we have, calling the total resistance R,

$$R = \int \frac{rg}{2} (\alpha^2 - \varphi^2) \sqrt{\frac{l}{g}} \frac{-d\varphi}{\sqrt{\alpha^2 + 2m\alpha - 2m\varphi - \varphi^2}}.$$

Integrating between the limits α and $-\alpha$, and putting $\sqrt{\frac{l}{g}} = \frac{1}{\pi}$,

$$R = \frac{rg}{2\pi}\left[\left(\frac{\alpha^2 - 2m\alpha}{2}\right)\left(\frac{\pi}{2} + \arcsin\frac{\alpha - m}{\alpha + m}\right) + (\alpha + 3m\sqrt{m\alpha})\right].$$

Since m is very minute in comparison with α, we may make $\frac{\alpha - m}{\alpha + m} = 1$, and also neglect $-2m\alpha$. The small positive term at the end becomes insensible, when multiplied by the general coefficient, in which k^2 is a divisor—the term itself being insignificant compared with π, with which it is connected by the sign $+$. The errors thus introduced, besides being insensible, are in opposite directions, and nearly balanced. The simplified expression is then,

$$R = \frac{rg\alpha^2}{4} = \frac{g^2 l \alpha^2}{2k^2}.$$

This resistance extends over the whole arc of vibration; but the maintaining power acts effectively only between the limits β

* This computation supposes the resisting surface to be plane. The actual value of k will vary with the form of the pendulum; and will ordinarily be considerably greater than it is here found to be. The disturbing effects upon the pendulum, deduced further on, will therefore be materially less than represented; since a less impelling force will be required to maintain the motion than the calculation exhibits. The actual value of k may be pretty nearly ascertained for bodies of regular shape, by considering the inclination of their surfaces to the direction of motion.

and $-\beta$, or during the time found by integrating the expression already given for $d\,t$ between the same limits. Then,

$$mg(t-t') = \frac{2mg}{\pi}\left[\frac{\beta}{\alpha} + \frac{1.\beta^3}{2.3\alpha^3} + \frac{1.3\beta^5}{2.4.5\alpha^5} + \frac{1.3.5\beta^7}{2.4.6.7\alpha^7}, \text{ \&c.}\right] \quad (4)$$

Or, putting S for the sum of the series within the brackets,

$$mg\,(t-t') = \frac{2\,mg\,S}{\pi}, \text{ which must equal } R.$$

Therefore,

$$\frac{2\,mg\,S}{\pi} = \frac{g^2\,l\,a^2}{2\,k^2}; \text{ and } 4\,Sk^2\,m = gl\,a^2\,\pi,$$

Whence,

$$m = \frac{w\,a}{M\,l} = \frac{g\,l\,a^2\,\pi}{4\,S\,k^2}; \text{ and } w = \frac{Mgl^2a^2\pi}{4\,a\,S\,k^2}. \quad (5)$$

The foregoing series rapidly converges, and if $\beta = \cdot 707\,\alpha$, its sum is $\frac{1}{4}\pi$. Putting α, the length of the pendulum arm, measured at right angles to the pendulum rod from the centre of motion, = 3 inches, and employing for the other symbols, the values heretofore given, we shall obtain for m and w the numerical values,

$$m = \cdot 000001597.$$

$$w = 2{\cdot}914 \text{ grains, or 3 gr. nearly.}$$

Returning to the expressions (1) and (2), with the value of m thus determined, and still employing for β the value $\cdot 707\,\alpha$, the sum of the series within the brackets in (1) will be found to be 1·384. And therefore $\Delta\,T$, or the daily acceleration, will be,

$$\Delta\,T = \frac{2 \times 86400 \times \cdot 000001597 \times 1{\cdot}384}{3{\cdot}14159 \times \cdot 035001597} = 3{\cdot}473 \text{ seconds.}$$

Whence it appears that this pendulum, in order to beat seconds, must be about three one thousandths of an inch longer than one entirely free.

In order to investigate the liability of this pendulum to change of rate, we must observe that, at a constant temperature, it is impossible that there should be a change of rate without a change of the arc of vibration; and further, that there is no cause in operation to change the arc, except variations of density in the air. In expression (5) we observe that a^2 varies as k^2; but it is evident that k^2 varies inversely as the density of the atmosphere. Or, putting D for the density,

$$a^2 \propto k^2 \propto \frac{1}{D}.$$

$$2\,a\,d\,a \propto 2\,k\,d\,k \propto -\frac{d\,D}{D^2}.$$

Hence,

$$\frac{2\,d\,\alpha}{\alpha} \propto \frac{2\,d\,k}{k} \propto -\frac{d\,D}{D}.$$

And,

$$\frac{d\,\alpha}{\alpha} \propto -\frac{d\,D}{2\,D}.$$

Putting the mean density of the air = 1, and substituting a finite difference for $d\,D$, we shall find that the corresponding finite difference of α will be but half as great in proportion to the entire arc, as the fractional change of density. If, therefore, under a constant temperature, the mercury in the barometer rise or fall one inch, or a change of density occur equal to one thirtieth of the mean, the arc of vibration will change one sixtieth part of the whole; that is to say, if the value of α is 2°, the arc will fall off, or increase to the amount of 2′.

To compute the effect of such a change upon the quantity ΔT, we may regard the series in equation (1) as being sensibly constant, and then, representing the whole expression, except the denominator of the coëfficient fraction, by Q, and omitting the insignificant term m from the denominator, we shall have,

$$\Delta T = -\frac{Q}{\alpha}, \text{ and } d\,\Delta T = \frac{Q\,d\,\alpha}{\alpha^2}.$$

Substituting $-\alpha\,\Delta T$ for Q and reducing, we have,

$$d\,\Delta T = -\frac{\Delta T\,d\,\alpha}{\alpha},$$

which, in the extreme case supposed above, gives a diminution of the daily acceleration equal to ·058 sec. This change is, unfortunately, in the same direction as that of the circular error: but it is proportional to the quantity ΔT itself, which is directly as the maintaining power; which, again, as appears from equation (5), is as the square of the arc. Hence, therefore, the importance of reducing the arc of vibration, and the near approach to insensibility of the errors arising from its variations, when it is small. Were the arc only 1° on each side of the vertical, the error would be between one and two hundredths of a second per day. Were it half a degree, the clock, from this cause, would not be an entire second in error in nine months.

The chief object had in view in the construction of the electric clock herewith exhibited, has been to secure the reduction of the arc of vibration. The work having just been completed, opportunity has not yet been allowed for experimentally deciding the question how great a reduction of arc is practicable; but the principle of the mechanism exacts no larger motion than

may be necessary to make and break electric contacts. The reduction of the arc α may be equally effected by either of two methods—either by reducing the impulse weights, or by shortening the duration of the impulse. When the arc is considerable, the former method appears preferable; when it is very small, there is not much to choose between the two; except that, by constantly reducing the impulse weights, they may perhaps become inconveniently small.

To return to the subject of compensation for variations of temperature, it may be observed that, while every pendulum is liable to be disturbed by the forces other than gravity acting upon it, and while these forces are not all of them subject to law, so that their effects can be exactly predicted and allowed for, it is not surprising that methods of compensation theoretically good should have failed to satisfy in practice. In the electric clock here presented, should its performance accord with expectation, and should it be found practicable to reduce the arc of vibration as far as it is at present believed to be, there will evidently be no sensible change of rate arising from any cause whatever, except expansion or contraction. If then the rate does actually change, the cause of error will be directly indicated; and the proper mode of correction may be made a subject of intelligent study.

A doubt has been already intimated above, whether the complaint made of the performance of the mercurial compensation, and of the glass jars as connected with it, is well founded. If the pendulum rod descends into the mercury, it would seem that there could be no great difference in the fluctuations of the temperature of the two metals. As the changes come from without it will be the rod which will be most directly exposed to them; but the capacity of the mercury for heat is so much less than that of steel, that its changes take place with correspondingly greater rapidity. If the smaller bulk of the rod in proportion to its surface, be in its favor, the remedy would be to make the rod larger, or to dispose the mercury in an annular vessel. But, at any rate, it is easy to make the containing vessel of iron according to the plan of Mr. Dent; and if this is done, and the expedient last suggested is adopted, of introducing the mercury into the annular space between two cylinders, it would seem that the mercurial compensation might be made quite perfect. As a final security against irregularities in the receiving of heat or parting with it, the entire surface both of the rod and of the containing vessel might be made uniform in character: which is done in the present clock by gilding. For a comparison of the performance of the compensation in glass and in iron, different jars are provided, which will be substituted for each other at intervals of several months. A brass cover, externally gilded, is also

provided, to be placed over either the iron or the glass jar, for the purpose of observing the effect of change of external surface.

These are some of the arrangements which have been made for future use in the experimental examination of the question under consideration. They would not have been brought to the notice of the Association until after having been instrumental in securing some results, were it not for the fact that no other opportunity will occur of exhibiting the clock itself—its completion having taken place just as the Association are meeting—and the constructor being on the point of forwarding it to the University of Mississippi, where it belongs. The observatory of the university is now in progress of erection, and it will be some time before the large transit instrument which is to be provided will be set up. It will be practicable, however, with less perfect facilities, to make some of those observations for which this clock is designed; and the conclusions to which such observations may lead will be communicated hereafter.

ART. XXIII.—*Enumeration of Ferns collected by Mr. Charles Wright, in Eastern Cuba in* 1856–7; by DANIEL C. EATON.

THIS enumeration has been prepared for the benefit of the subscribers to Mr. Wright's Cuban collections. It is unavoidably imperfect, since my materials for identifying tropical ferns are scanty, and, indeed, I should not venture to print it, were it not for the kindness of Sir W. J. Hooker, who has examined and named for me several of the more obscure species. Mr. Wright is again collecting in Cuba: after his return a supplement to this list will probably be published.

774. Hemionitis palmata, *Linn.*
775. Antrophyum subsessile, *Kunze, Analect.* p. 29, t. 19.
776. A. lanceolatum, *Kaulf.*
777. Gymnogramme tartarea, *Desv.*
778. G. sulphurea, *Desv.*
779. G. trifoliata, *Desv.*
780. Xiphopteris serrulata, *Kaulf.*
781. Meniscium sorbifolium, *Willd.; Langsd. and Fisch. Ic. Fil.* t. 4.
782. M. sorbifolium, *Swartz.* This is probably but a variety of the last with narrower pinnæ.
783. Gymnopteris aliena, *Presl.* (Acrostichum alienum, *Swartz.*)
784 & 785. Olfersia cervina, *Kunze; Hook. Filices Exoticæ*, t. 43. (Acrostichum cervinum, *Swartz.*)
786. Polybotrya osmundacea, *Humb. & Bonpl.?*
787. Lomariopsis sorbifolia, *Fée, Hist. des Acrostichacées*, p. 69, var. ? Perhaps this Fern deserves to be described as a new species, but I am unwilling to name and describe it from my present scanty materials.

788. Gymnopteris nicotianæfolia, *Presl.* (Acrostichum nicotianæfolium, *Swartz.*)
789. Elaphoglossum ciliatum, *T. Moore, Index Filicum*, p. 8. (Acrostichum Preslianum, *Fée, 2me. Mém.* p. 46, t. 24; *Hook. in litt.*)
790 & 791. E. latifolium, *J. Smith, Catal. Kew Ferns*, p. 3. (Acrostichum latifolium, *Swartz; Hook. Fil. Exot*, t. 42.)

The name Elaphoglossum is retained for this genus because there was no representative of it in the original genus Acrostichum, (*Linn. Amœn. Acad.* i, p. 268,) which contained only two real Acrostichaceæ, A. aureum and A. lanceolatum (Leptochilus Linnæanus, *Fée*), the former of which must keep the name Acrostichum.

792. Hymenodium crinitum, *Fée, 2me. Mém.* p. 90. (Acrostichum crinitum, *L.; Hook. Fil. Exot.* t. 6.)
793. Elaphoglossum.
794. E. piloselloides, *T. Moore, l. c.* p. 13. (Acrostichum piloselloides, *Presl.; Hook. l. c.* t. 29.)
795. Goniophlebium incanum, *J. Smith.* (Polypodium incanum, *Swartz.*)
796. Pleopeltis angustifolia. (Polypodium elongatum, *Mettenius, Polypod.* p. 88, non Pleopeltis elongata, *Kaulf.*)
797. Campyloneuron tæniosum, *Fée, Gen. Fil.* p. 258. (Polypodium tæniosum, *Willd.; Mettenius, l. c.*, p. 52.)
798. Goniophlebium piloselloides, *J. Smith, in Hook. Jour. Bot.* 4, p. 56. (Polypodium piloselloides, *L.*)
799. Campyloneuron.
801. C. Cubense, *Fée, Iconogr.* p. 14 and 129, t. 3. (Polypodium tæniosum, var. *Mettenius, l. c.*)
803. Phlebodium aureum, *R. Br.; Hook. Gen. Fil.* t. 112. (Polypodium aureum, *L.* Chrysopteris aurea, *Link, Fil.* sp. p. 121.)
804. Goniophlebium neriifolium, *J. Smith, l. c.* (Polypodium neriifolium, *Swartz.*) *Hook. in litt.*
805. Polypodium sororium, *H. B. K.*
806. P. pectinatum, *L.*
807. P. Funiculum, *Fée, Iconogr.* p. 12, t. 18.
808 & 810. P. suspensum, *L.; Hook. in litt.*
809. P. Camptoneuron, *Fée, Gen. Fil.* p. 237, *Iconogr.* p. 60, t. 23.
811. P. trichomanoides, *Swartz.*
812. P. hastæfolium, *Swartz; Hook. & Grev. Ic. Fil.* t. 203.
813. Goniopteris reptans, *Presl.* (Polypodium reptans, *Swartz.*)
814, 816 & 865. Polypodium (Phegopteris) sanctum, *Swartz.*
815. Lastrea pubescens, *Presl.* (Aspidium pubescens, *Swartz; Vid. Hook. & Grev. Ic. Fil.* t. 162.
817. Goniopteris tetragona, *Presl.* (Polypodium tetragonum, *Swartz; Schkuhr, Fil.* t. 18^{b}. Phegopteris tetragona, *Metten. Fil. Lips.* p. 84.) *Hook. in litt.*
818, 819 & 822. Lastrea patens, *Presl.* (Aspidium patens, *Swartz.*)

This is a common and most variable fern in the Southern States from Florida to Louisiana and Texas. It resembles Nephrodium molle, and was mistaken for that species by *Kunze.* (*Am. Jour. Sci.* vi, p. 83.)

820. L. contermina, *Presl.* (Aspidium conterminum, *Willd.*) *Hook. in litt.*

823. Nephrodium deltoideum, *Desv.* (Aspidium deltoideum, *Swartz; Metten. Phegopt. und Aspid.* p. 93.)

824. N. Skinneri, *Moore, Index Filicum*, p. 104. (Aspidium Skinneri, *Hook. Ic. Pl.* t. 924.)?

825. N. stenopteris. (Aspidium stenopteris, *Kunze, Fil.* 2, p. 48, t. 120.)

826. Nephrolepis exaltata, *Presl.*

827. Goniophlebium loriceum, *Fée, Gen. Fil.* p. 255. (Polypodium loriceum, *L.*)

829. Polystichum triangulum, *Fée.* var. (Aspidium triangulum, *L.* var. laxum, *Hook. Fil. Exot.* t. 33. Polystichum ilicifolium, *Fée. Gen. Fil.* p. 279, *Iconogr.* p. 21, t. 6.

830. Lastrea Melanochlamys, *Moore, l. c.* p. 96. (Aspidium Melanochlamys, *Fée, Gen. Fil.* p. 294.)

831. L. exculta, *Moore, l. c.* p. 91. (Aspidium excultum, *Metten. Phegopt. und Aspid.* p. 69. Aspidium lætum, *Moritz.*) *Hook. in litt.*

832. Polystichum platyphyllum, *Presl.* (Aspidium platyphyllum, *Willd.* Phegopteris platyphylla, *Metten, l. c.* p. 122.)

833. Aspidium cicutarium, *Swartz; Metten. l. c.* p. 117.

834. A. macrophyllum, *Swartz; Metten. l. c.* p. 122.

835. A. trifoliatum, *Swartz.*

836. Oleandra nodosa, *Presl.*

837. Asplenium serratum, *L.; Hook. Fil. Exot.* t. 70.

838. H. marginatum, *L.; Hook. l. c.* t. 73. (Hemidictyum marginatum, *Presl.*)

840. A. serra, *Langsd. & Fisch. Ic. Fil.* t. 19.

842. A. dimidiatum, *Swartz.* (A. zamiæfolium, *Kunze, Fil.* p. 103, t. 48.)

833. A. falcato, *Lam.*, affine.

844. Fadyenia prolifera, *Hook. Gen. Fil.* t. 58, B; *Fil. Exot.* t. 36. (Aspidium Fadyenii, Mettenius, *Fil. Hort. Lips.* p. 95. Asplenium proliferum, *Swartz.*)

845. Asplenium, salicifolio, *L.* affine.

846. Diplazium grandifolium, *Swartz.; Hook. in litt.*

847. Diplazium.

848. Asplenium bidentatum, *Willd.*?

849. A. auricularium, *Desv.*

850 & 851. A. rhizophorum, *Swartz; Hook in litt.*

852. A. bisectum, *Swartz.*

853. A. dentatum, *L.; Hook. & Grev. Ic. Fil.* t. 72.

854. A. formosum, *Willd.; Hook. Fil. Exot.* t. 16.

855 & 856. A. cicutarium, *Swartz.*

857. A. fragrans, *Swartz; Hook. in litt.*

858 & 859. Onychium strictum, *Kunze, Fil.* 2, p. 11; *Hook. Sp. Fil.* 2, p. 123.
860. Gymnogramme leptophylla, *Desv.*
861. Asplenium pumilum, *Swartz.*
862. Didymochlæna sinuosa, *Desv.* (D. truncatula, *J. Smith.*)
863. Blechnum occidentale, *L.*
864. Lomaria decrescens, *Fée, Gen. Fil.* p. 68, *Iconogr.* p. 24, t. 9. (L. attenuata, *Willd., ex Hook. in litt.*)
865. Vittaria lineata, *Swartz*—(the longer specimens.)
865bis. V. sp. ign.—(the shorter specimens.)
866. Pleurogramme immersa, *Fée. 3me. Mém.* p. 37, t. 4. *Hook. in litt.*
867. Pteris pedata, *L.; Hook. Sp. Fil.* 2, p. 208. *Fil. Exot.* t. 34. (Doryopteris pedata, *J. Smith.*)
868. P. leptophylla, *Swartz; Hook. Sp. Fil.* 2, p. 216. (Litobrochia leptophylla, *Fée, Gen. Fil.* p. 135.)
869. P. mutilata, *L.; Hook. l. c.* p. 164, t. 131.
870. P. denticulata, *Swartz; Hook. l. c.* p. 215. (Litobrochia denticulata, *Fée, l. c.*)
871. P. longifolia, *L.*
872. P. aquilina, *L.* var. caudata, *Hook. l. c.* p. 196.
873. P. aculeata, *Swartz; Hook. l. c.* p. 224. (Litobrochia denticulata, *Fée, l. c.*)
874. Adiantum macrophyllum, *Swartz; Hook. Fil. Exot.* t. 55.
875. A. trapeziforme, *L.*
876. A. tenerum, *Swartz.*
877. A. concinnum, *H. B. K.*
878. A. fragile, *Swartz.*
879. A. pulverulentum, *L.*
880. A. cristatum, *L.; Hook. in litt.*
882. A. villosum, *L.*
883. Pteris laciniata, *Willd.; Hook. Sp. Fil.* 2, p. 176, t. 132.
886. Polypodium (Phegopteris) barbatum, *Kunze in Linnæa*, 9, 52. *Hook. in litt.*
887. Cheilanthes microphylla, *Swartz.*
888. Hemitelia horrida, *R. Br.; Hook. Sp. Fil.* 1. p. 30, t. 15; *Fil. Exot.* t. 69.
889. Alsophila.
890. A. muricata, *Hook. in litt.*
891. Cyathea Serra, *Willd.* var.?
892 & 893. C. arborea, *Smith; Hook. Sp. Fil.* 1, p. 17.
894. Hypolepis repens, *Presl.*
895. Dicksonia cicutaria, *Swartz.*
896. Davallia polypodioides, *Don.*
897. Dicksonia Plumieri, *Hook. Sp. Fil.* 1, p. 72.
898. Davallia aculeata, *Swartz.*
899. D. uncinella, *Kunze, Fil.* 2, p. 96, t. 140.
900. Trichomanes crispum, *L.*
901. T. macroclados, *Kunze, l. c.* p. 72, t. 130.
902. T. Radicans, *Swartz; Hook. in litt.*

903. T. anceps, *Hook. Sp. Fil.* 1. p. 135, t. 40?
904. Hymenophyllum sericeum, *Swartz.*
905. H. hirsutum, *Swartz.*
906. 907 & 908. Trichomanes pyxidiferum, *L.* var.
909. T. angustatum, *Carm.; Hook. & Grev. Ic. Fil.* t. 166; *Hook. in litt.*
910. Metzgeria fucoides, *Nees & Montagne.*
911. Trichomanes membranaceum, *L.*
912 & 913. T. muscoides, *Swartz.*
914. T. apodum, *Hook. & Grev. Ic. Fil.* t. 117.
915. T. reptans, *Swartz.*
916. T. holopterum, *Kunze, Fil.* 1, p. 185, t. 77?
917. Hymenophyllum asplenoides, *Swartz.*
918. Hymenophyllum.
919. H. undulatum, *Swartz.*
920. H. abruptum, *Hook. Sp. Fil.* 1, p. 88, t. 31.
921. Gleichenia pubescens, *Willd.*
922. G. dichotoma, *Willd.*
923. Nephrodium Serra, *Desv. Ann. Linn.* 6, p. 253. (Aspidium Serra, *Swartz.*)
924. Danæa nodosa, *Smith; Hook. & Grev. Ic. Fil.* t. 51.
925. Lygodium Poeppigianum, *Presl. Suppl. Tent.* p. 103?
926. Schizæa dichotoma, *Swartz.*
927. Rhipidopteris peltata, *Fée. 2me. Mém.* p. 78. (Acrostichum peltatum, *Swartz.*)
928. Anemia adiantifolia, *Swartz.*
929. A. Breuteliana, *Presl. l. c.* p. 90. (A. Mandioccana, *Hook. Gen. Fil.* t. 90.)
930. Ophioglossum vulgatum, *L.* (Ophioglossum reticulatum, *L.*)

Dr. J. D. Hooker in the Flora of New Zealand unites all the species of Ophioglossum proper, and Sir W. J. Hooker, in his and Dr. Arnott's Flora of Great Britain, says he is ready to acknowledge the correctness of this view.

931. Psilotum triquetrum, *Swartz.*
932. Lycopodium cernuum, *L.*
933. L. reflexum, *Swartz; Hook. in litt.*
934. L. linifolium, *L.*
935. L. verticillatum, *L.*
937. L. taxifolium, *Swartz.*
938, 939 & 940. Species of Selaginella.

ART. XXIV.—*Some observations on the Motions of certain Winding Plants;* by WM. H. BREWER, Prof. of Chemistry in Washington College, Pa.

IT has long been recognized as a general law, that green plants during their growth grow towards the light, but all the botanical works that have come under my observation, which speak of winding plants and tendrils in this connection, speak of them as forming, practically, an exception to this law, that is, that they turn towards some "*dark*" or "opaque" object. That they do turn towards a solid support has long been observed, the fact is undisputed, and the cause of this motion, instinctive as it were, towards some solid around which they may twine has always been given, directly or inferentially as the absence of light, or more properly the opacity or non-luminous character of the support. I have been unable to find any account of experiments on this property of certain plants or of certain organs of plants further than merely to show the fact, that it exists.

During the summer of 1855 I made some observations on the growth of a hop vine (*Humulus*) to ascertain more precisely the relations between the rate of growth at different hours of the day, and the temperature, clearness and other atmospheric conditions. To effect this the vine was measured at stated hours several times each day, and the better to do this it was not allowed to wind around, but was trained up one side of a smooth pole. Incidental to the desired observations, it was noticed that during the heat of the day, although the plant sometimes grew several inches, it grew towards the light with only a very slight tendency to wind around the pole, while during the night, or on cold days, while the rate of growth was slower it would assume the spiral and cling closely to the support. On one occasion, when a number of plants were only from one to two feet high, a slight fall of snow took place which remained a day or more, and in a few hours, all the plants which had sprung up from the ground and remained perfectly erect until this time, inclined at a high angle towards a lattice which was artificially heated.

It was also found that they would climb a transparent glass tube almost or quite as readily as an opaque stick. These and similar observations at other times suggested to me that the cause of the motion towards a support was not owing to any influence of light, or its absence, but rather to *heat*, and to elucidate this subject a series of experiments were made at Ovid, N. Y., during the last summer.

These consisted in the main of presenting a warm and a cold support to some winding plant, and then observing if it manifested any preference. The plants experimented on were the

common Lima bean (*Phaseolus lunatus* L.) and the common morning glory, (*Convolvulus purpureus* L.) The general plan was to keep the plants in a closed room during the day and early part of the evening, where the air could be kept at a rather high and nearly constant temperature, and then remove them for the night into another room where the temperature was several degrees lower than the first, where the warm and cold supports were presented to them. This room was also closed and darkened that neither currents of air nor morning light should interfere with the accuracy of the experiments.

1.

For the supports tin tubes were used, of the shape given in the figure, having a funnel *a*, at the top, and an elbow *b*, at the bottom, forming an obtuse angle. These were about an inch in diameter, similar in size and shape, and the vertical part painted black. These could be kept cool by filling with cold water, and if desired by placing ice in the funnel *a*, and could be warmed and kept at any desired temperature higher than the air, by a small spirit lamp placed under the end *c*. For the use of glass and other materials, an elbow of tin was employed, and then the straight tubes fitted with a cork. To test the effect of colors, tin tubes were painted of various colors, and in some cases colored paper was pasted around them. White, black, red, pink, green, blue, and yellow, were tried. When in use the tubes were held in a nearly vertical position, about five inches apart, one filled with well-water a few degrees colder than the surrounding air, the other filled with warm water and kept heated to any desired temperature by a spirit lamp, generally from 5° to 12° Fahr., above the temperature of the air in the room. The plant was placed at the beginning of the experiment so as to be midway between the two tubes, not exactly parallel with them, but crossing their plane at a low angle. It was allowed to remain without disturbance from 9 P. M. until 7 A. M., and its position, the temperature of the air and the water in the tubes and other conditions accurately noted at the beginning and close of each experiment.

Many preliminary experiments were made to devise means to avoid the various causes of interference, and to test and perfect the apparatus, and they so far succeeded that I consider the results given as reliable. After these, a series of fifty-two experiments were carefully made, of which nineteen were with Convolvulus, and twenty-three with Phaseolus. These gave in thirty-six cases results confirmatory, that is, the vines turned to

or towards the warmed tube, in fourteen cases they showed no especial preference, and in only two cases did they turn to the cold tube. In these fifty-two experiments, the right tube was heated twenty-five times, and the results were nineteen confirmatory and six indifferent; the left tube twenty-seven times, and the results seventeen confirmatory, eight indifferent and two contradictory, (that is, turned to the cold tube). In both of these latter cases the nights were exceedingly hot (one was 84° F.) and the experiments were in a room in which the sun had shown a part of the day and the walls had become heated, so that on closing the room the temperature rose during the night several degrees; the heat radiated from the walls doubtless effected the results. During the cooler nights, or when the temperature was below 65° F., the results were most marked, and generally in the morning the point of the vine, left the evening before midway between the two tubes, would be found not only moved towards the heated tube but would be closely twining around it, the point of growth lying closely against the surface. The right and left tubes were in turn heated on alternate nights and also they were made to exchange places occasionally. As both of the plants experimented on wind to the left, (the right according to Bischof) it will be readily seen that it makes much difference which tube is heated, when the plant is placed in the position relative to them which I have described, in the form the spirals will assume.

Thus, let *a* and *b* be the sections of the two tubes, and *c* the extremity of the plant *c d*, at the beginning of the experiment. Then if *a* be heated (the one I have assumed as the left tube in the description) the plant will gradually assume the position of the dotted line *m m*, by simply turning to the left. If however, the right tube, *b*, be heated, the plant will take the direction of the dotted line *n n*, by first rising vertical and then passing behind and around the tube.

2.

The room in which all the experiments (with the tubes) were conducted had but one window, opening west, which at night was carefully closed and darkened. In half an hour, sometimes in a few minutes, after the light had been admitted in the morning, the growing point of the vine would slightly relax the hold with which it would press against the support, and then during the day its growth would be towards the light. During this period, the tendency to grow in the direction of the light was vastly greater towards the warmed tube; in fact, the Phaseolus seemed to be entirely insensible to the latter during this time, and the Convolvulus nearly so.

I found that the Phaseolus, if grown in a room in which the temperature was high and nearly constant, not falling more than 3° or 4° F. during the night, would wind about a support in such very long loose spirals that it could not retain its position, but would slide down from time to time, and this same plant, when allowed the influence of cooler nights, would then wind in shorter spirals and cling with its accustomed tenacity to the smooth stick which served as a support. Furthermore, I found that by placing a plant in such a position that the sun could shine on its growing extremity, but not on its support, and changing it occasionally to keep up the conditions, turning it so that its tendency to grow towards the light was in opposition to that of its winding, and then keeping it at night at nearly the same temperature that it had during the day, I could entice it entirely away from the support until a length of several feet of the vine was pendant and unsupported.

These indicate the same fact sustained by the experiments with the tubes, viz., that plants wind best when the support is warmer than the air. This condition is fulfilled in nature at night, as the solid absorbs the sun's rays by day and cools more slowly than the surrounding air by night. I am aware that such plants will wind in nature around cold supports, such as growing plants of other species, but I doubt if their first direction towards them, before the contact is more than accidental.

There appears to be much difference in the force with which different species of winding plants assume the spiral. The Convolvulus seemed much more sensitive to the influence of heat than the Phaseolus, before it was in contact with anything, and much more independent of it afterward, for when once in contact with a support it could not be induced to again leave it, and would follow a piece of twine or slender rod apparently as readily as a more solid material. Many experiments seemed to indicate that *contact* with the support modifies the force with which plants assume the spiral, that in fact, although the fibres of the plant are somewhat spiral about its axis before contact, afterwards, these spirals are shorter, and only then will the whole plant assume a spiral form as if to enclose something in its turns.

This was beautifully shown by introducing the end of a vine into a thin glass tube at night; the fibres of the plant would assume a shorter spiral and sometimes the plant itself would wind around on the inner surface of the tube in the same form and direction as if it had enclosed some cylinder in its turns, while plants not so treated would remain nearly straight and their fibres less spiral.

The experiments with tubes of various colors gave no results materially different from the others.

These experiments were more striking than was anticipated, but were prosecuted under difficulties which prevented their being completed.

They are intended as the preliminaries to more extended and complete investigations in the same direction, to be continued at some future time, embracing the interesting question, whether tendrils are influenced by the same causes and follow the same law, also some things relating to the direction of winding plants, the length of their spirals compared with their diameter, the direction of the spiral growth of various trees, &c. Some observations have already been made on all of these subjects except that of tendrils.

The experiments performed, indicate, I think,

1st. That during the day winding plants like others grow towards the light.

2d. That they possess the property of turning towards some solid support.

3d. That this is more manifest by night than by day, and the most so on cool nights following hot days.

4th. That this is not controlled by any influence of light or its absence, exerted by the support.

5th. That heat is the controlling cause, and that such plants will only turn (unless it be accidentally) towards a support, the temperature of which is higher than that of the surrounding air.

6th. That the color and material of the support exert no influence further than that they influence the radiation and absorption of heat; and

7th. That when such plants are in actual contact with some support, the tendency to wind spirally around it is much greater than they manifested in order to reach it.

ART. XXV.—*On some Anomalies in the Florida Gulf Stream, and on their further Investigation;* by Lieut. E. B. HUNT, Corps of Engineers, U. S. A.

(Read before the American Association for the Advancement of Science, at the Baltimore Meeting, May, 1858.)

THERE is perhaps no portion of the ocean waters which has been so imperfectly studied, in proportion to its importance, as that from the west end of Cuba through the Gulf of Florida. The whole commerce of the Gulf of Mexico is directly concerned in whatever investigations shall more accurately define the currents or other physical peculiarities of that portion of the Gulf Stream area from the line joining Cape Antonio to Cape Catoche, and the latter with the Tortugas, thence to the eastward through that grand channel bounded on the north and

west by the Florida Reef and mainland, and on the south and east by Cuba and the Bahama banks. As it is the natural outlet for the products of the entire gulf coast and of the valley of the Mississippi, the present commerce following this route, vast as it is, must ultimately be so far exceeded, that it will seem almost insignificant in comparison with that which another half century will direct through this channel. The character of this navigation, no less than its amount, is such as to demand the most careful study of the currents, by which it is so largely influenced. All are so familiar with the immense annual losses to commerce by wrecks and disasters on the Florida Reefs and Bahama Banks, that a simple reference to the fact will abundantly vindicate the importance of carefully gathering whatever knowledge can give greater safety to this navigation. The high extra premiums for marine insurance by this Florida channel route, afford another striking testimony to the risks of this navigation; but I think it right to remark here, that, from the best information I could obtain at Key West, the Florida channel insurance rates are very much too high, and are annually giving exorbitant profits to the insurance companies. This makes it the interest of these companies to exaggerate the dangers of this passage; and it is believed that they are, on this account, less averse to wrecks, and less strict in distinguishing collusive or fraudulent wrecks than they should be. A reduction of rates must soon be made, if we may judge from the fact that owners are, to a considerable extent, becoming their own insurers, in preference to paying the established rates. The new light-houses and coast-survey beacons have added much to the security of this route, and the business of wrecking at Key West is, on the whole, diminishing, although commerce is of course increasing. Making all due abatement for the exaggerations of the terrors of the Florida Straits, and for the increased aids to navigation, there still remains a very serious annual marine loss, due almost entirely to the imperfect acquaintance of navigators with the peculiarities of the route, and especially with the currents. Misled by false or imperfect views about the Gulf Stream, and other currents prevailing here, sea captains are frequently so unfortunate as to run directly on the reefs, while they suppose they are well out in the channel way. I cannot but think that a system of reef pilotage, properly organized and well conducted, would lead to a great reduction in the number of casualties. By taking and leaving well-trained pilots at the entrance to, and exit from, the region of danger, the numerous casualties due to the imperfect knowledge of sea captains would be in great part obviated. Skillful pilots, constantly engaged in taking vessels through the channel, would grow more and more certain of all the essentials for secure passages, until it would become a gross

offence to lose a vessel, except from causes truly extraordinary. It is obvious that such pilots should be so situated that no possible advantage could accrue to them in case of wreck, while a premium should be awarded for each safe pilotage. Were such a system in full operation, it would be a proper rule that a vessel failing to take a reef pilot should forfeit its insurance, except when no pilot could be procured. A great difficulty in accomplishing this plan except by the combined action of the insurance companies, is found in the fact that three national jurisdictions enter the field of pilotage.

A first essential for giving greater security to the Florida channel navigation is a more correct determination of the currents by which vessels are affected during the transit. Having spent the last winter at Key West, I was led to inquire about these currents with some particularity, and, as a result, was brought to the opinion that the prevalent views are very seriously at fault. There are many facts quite incompatible with the common notions of a vast current constantly sweeping around the Gulf of Mexico, and thence pouring in full volume through the Gulf of Florida. I will here cite some notes of testimony given me by various persons specially acquainted with the matters in view. They were questioned more particularly with reference to facts and opinions touching a southwesterly current prevailing more or less between the easterly Gulf Stream and the Florida reef. Such a counter or eddy current is definitely indicated on Jeffrey's map of 1794, by a dotted line, above which is written, "North of this line is a current setting southwestward, unless when the wind is at north or east, which winds admit of no southwest;" and, "South of this line the current of the Florida stream sets always northwardly."

Capt. Geiger, who for over thirty years has been observing the waters of this vicinity, most of that time having acted as a pilot off Key West harbor, and who is perhaps better acquainted than any other person with the currents there prevailing, gives the following statement of facts.

A strong north or northeast wind keeps the Gulf Stream back, and makes a westerly current near the shore. During June, July, and August, the westerly current prevails more than the easterly current from five to fifteen miles from the reef. The direction of the current depends mostly on the wind. The westerly current prevails for from one third to two fifths the entire time from year to year, for from two to fifteen miles outside the reef off Key West. He has known it twenty-five to thirty miles off Sand Key. When the Gulf Stream is strongest on the Cuba shore, the westerly current is strongest on the north side; and when it is weakest along the Cuban shore, the Gulf Stream sets close along the reef. He has found the westerly current as

far up as Carysfort, but not frequently, and not broad or strong. This current broadens from Carysfort to the westward, and continues about constant along its course. The tides on the two sides of the reef are about six hours apart, on an average; but set, on the whole, as much one way as the other over the reef. Sometimes there is a narrow easterly current for a mile from the reef; then a westerly current, and then the Gulf Stream. Both the United States steamers Susquehanna and Wabash were set westwardly by the current about eight or ten miles during the past season. A considerable number of the Gulf traders know of the westerly current, and make more or less use of it in navigating westwardly. When running with the wind the water is smooth, and rough when running against it. After northers, the westerly current is to be expected. Sometimes, in crossing to Havana, no Gulf Stream indications are found; and sometimes a westerly current is found along the north shore of Cuba. Notwithstanding Capt. Geiger's long observation of these currents, he says that he is quite unable to reduce them to rule, or in any way to know before hand how the current will be found to set. He asserts that the Gulf Stream sets from the vicinity of Cape St. Antonio, northeasterly through the Florida channel, and that the main stream does not make the circuit of the Gulf of Mexico as generally supposed.

Captain Richardson, pilot of the Coast Survey surveying steamer Corwin, says, in substance, as follows: The westerly current appears irregularly chiefly in the winter, but sometimes during the prevalence of the regular trades. It extends from ten to fifteen miles off from Sand Key, sometimes running as much as two miles an hour. It never prevails over the reef proper. It sets for two months or so some winters. It spreads farther from the reef as it goes west. Has known it as far north as Carysfort, just outside the reef, and at Cape Florida even where the reef is narrow and deep, this current sometimes sets across it some two miles from shore, but is not very frequently found there. As it runs west it seems to increase in breadth. Off Indian Key he has known it to extend seven miles from the edge of the reef; at Bahia Honda it is sometimes ten miles, and at Sand Key, from ten to fifteen miles. In the winter of 1856–7 there was very little of this current. In crossing from Key West to Havana the Gulf Stream runs much stronger on the Cuban side. To some extent, navigators know this westerly current, and use it with great advantage when bound west. In one case in 1852 he knew of two vessels bound east past Tortugas which separated about 100 miles in twenty-four hours, by one captain knowing this current and the channel, while the other kept in the westerly current. The tide between the Quicksands and Tortugas sets flood N.N.E. and ebb S.S.E., differing from the charts.

Captain Wilson who has for several years been running on the vessel serving Fort Jefferson on Tortugas as a mail boat to and from Key West, says that for some three months prior to Feb. 11, 1857, there was a strong and decided westerly current on the north margin of the gulf, on the reef between Key West and Tortugas. It had then for some two months been constantly to the west. Running out from Tortugas on an E.S.E. or S.E. course, and tacking to the north or east of north when the point was reached, which in an ordinary gulf stream would bring him out somewhere from Sand Key light to six or seven miles west of it, which tack if there were no currents would bring him out abreast the Marquesas, he has six times in the last three months come out abreast the Quicksands, thus falling short of where he would have been had the water been still, by some eighteen to twenty miles, or some thirty miles west of Sand Key where he would have fetched during a full Gulf Stream current. It usually takes about twenty hours to run both branches of this tack. There is no appearance of any current on or within the reef either way except the set of the tides. When the westerly current is running, he finds it better to beat up within the reef than to attempt to cross over into the Gulf Stream. Mr. W. thinks the current sometimes extends half way across to Cuba. He says, this westerly current has prevailed more or less, every winter for seven years that he has been running between Key West and the Tortugas; but never so strong as this winter (1857–8), or for so long a time, probably not over a month in any previous case. He thinks the westerly current mainly disappears during the prevalence of the regular summer trades.

I was informed by General Totten that Com. Bainbridge told him, that in a voyage made by him some fifty years since, when he supposed himself in the Gulf Stream, west of Cape Florida, he found by known landmarks that he had drifted a considerable distance to the westward instead of to the eastward.

I am indebted to Mr. Charles Tift, of Key West, for the following notes:—

"In December, 1856 (I think), the barque Joseph Hale from Philadelphia for New Orleans, got ashore ten miles southeast from Cape Florida light-house. She had passed round the 'Isaacs,' and made the Orange Keys, steering for the Double-headed Shot Key's light. While looking out for the light, the ship apparently going seven knots, she struck, and proved to be in the position above stated.

"The ship Rockland from New Orleans to Boston was off the Pan of Matanzas at four o'clock (say March 25th, 1858), wind E.S.E., ship going per compass E.N.E., intending to sight Double-headed Shot Key light. At twelve o'clock saw what was supposed to be the light on Double-headed Shot Keys, and kept the

ship off to pass it on the gulf or western side. But the light proved to be the new one on Sombrero Shoal (just opposite), of which the captain had no notice, and she struck a shoal inside the main reef. A glance at the chart will show how far these captains were mistaken in their estimate of both the force and direction of the current.

"Some years since a fishing smack left Key West to go to Cape Florida. The wind was eastwardly, and after she had beaten to windward for some forty-eight hours, she stood in to make the land. She fetched twenty miles to the westward of the starting point, showing in this instance, a strong westerly current from the centre of the Gulf."

Mr. Tift adds that he "knows that the 'gulf current' sometimes, though rarely, runs strong to the eastward a mile or more *inside* of the reef (at Key West), but that the general set is westward for a *short* distance from the main reef." The idea, however, of a "strong westerly current" on this "edge," must be taken with many grains of allowance. A ship-master leaving the strong gulf current and approaching the margin, finds the set so reduced in its rapidity as to conclude that he has found the stream actually going westward. As stated above, this is only true to a very limited extent, or in other words the belt of westwardly current is very narrow.

I am indebted to Mr. Baldwin, collector at Key West, for a case in his own experience showing a westerly drift, and for some observations made specially valuable by his long and full acquaintance, not only with the matters in question, but with the navigators frequenting Key West.

In June, a few years since, Mr. B. made a passage in a fast-sailing brig from Mexico to Havana. After leaving Campeachy Bank, they made Tortugas Islands and took a departure about sunset, steering about southeast by east. About midnight it fell calm, and for five days they experienced only calms and occasional light airs from the south. On the sixth day there was a light wind from the east. The master, an experienced navigator and well acquainted with those waters, steered south, supposing he had drifted through the Gulf. On the morning of the seventh day he made land, which he supposed was somewhere near Matanzas, but which turned out to be near the Colorados, a reef off the west end of Cuba.

Again, in a voyage from St. Marks to Key West, Mr. Baldwin says, that being set by strong southwest currents in the Bay of Mexico, they fell to leeward and made the Tortugas Islands. Having an experienced pilot they ran through into the gulf between the Tortugas and the Quicksands. After beating to windward for three days they stood in, and found themselves six miles to leeward of where they entered the gulf. Satisfied that

they had to contend with a strong westerly current, the master consented to beat up inside the reef, and they reached Key West in thirty-six hours.

Mr. Baldwin says he has conversed with many intelligent ship-masters, with the Key West pilots, and with the masters of fishing smacks who are constantly crossing and recrossing the gulf to and from Cuba, and says that they assure him that no dependence can be placed on the Gulf Stream; sometimes it runs very much stronger than at other times in a northeast direction: that it very frequently runs in a southwest direction; and that at other times there is no current at all. Very frequently they experience an easterly current on the Cuba coast and the reverse on the Florida coast, and at other times a strong current in the centre. The current cannot be mistaken, as the change is perceptible to the eye.

Mr. Baldwin adds, "A great deal depends on the force of the wind. My own observation has satisfied me that the wind influences the set of the Gulf Stream; for instance, after a heavy northeast wind the stream sets to the northeast at a very rapid rate, and *vice versa*. Since my residence at Key West, I have known several vessels to be brought in from the northwest, having got into the Bay of Mexico, when supposing themselves east of Cape Florida."

He was assured by the master of a vessel from Honduras, and another from Central America, both stranded, that they had come round Cape Antonio, and after beating as they supposed in the gulf, aided by the Gulf Stream for a number of days, discovered land, and judging it to be the Bahamas, shaped their course through the gulf, and were stranded near the Cedar Keys.

These scraps of testimony might be much extended if necessary, but I suppose they fully suffice to show that we are still very far from possessing the knowledge the case demands. They clearly prove that there is enough westerly current in the Gulf of Florida to be of vast importance to navigation if its movements can be defined, and to constitute a great danger, if it is not known. Its variations are also well established, and should be known to navigators. I am also quite well persuaded, not only from actual testimonies, but from the fact that a coral bank extends above Cape Catoche, that at least a large part of the Gulf Stream turns to the northeast around the west end of Cuba, instead of making the circuit of the Gulf of Mexico. The effect of the earth's rotation, and of its own inertia, on the current coming north from the Caribbean Sea, would be to give it an eastward bend. It is also quite incompatible with the tendency of the westerly current to expand towards Tortugas, to suppose that the main Gulf Stream comes sweeping in from near the mouth of the Mississippi towards this point.

Before attempting to theorize on the cause of this westerly current, it is certainly very desirable that it should be more accurately defined. The effect of dragging by the Gulf Stream along its sides, may perhaps be to produce a deficiency of water behind, to be replaced by a return current of this degree of force, but it would certainly not call for such a vast body of westerly current as is vouched for in some cases, nor would it explain its alleged fluctuations. Some of the wrecks which have lately occurred seem due to a strong current setting through the Santarem Channel, and we may see in this a suggestion of a cause for the westward currents when these exceed the magnitude of a proper eddy. A Santarem current projected across the gulf, may be thrown down the reef, though I should not much expect such a result.

It will be well here to call attention to the refutation of the theory that the Gulf Stream owes its progress to a declivity resulting from heaping up waters in the Gulf of Mexico, which this parallel counter current affords. There is no evidence of any such elevation of the Gulf of Mexico as this theory calls for. On the contrary there is no such southeast current across from the Bay of Mexico, Barnes's Sound, &c.,—as such an elevation would inevitably create. The whole motion of a descending river in the sea, with its source in the Gulf of Mexico, seems to me quite untenable and conflicting with facts.

The natural conclusion from what has preceded is, that there is abundant need of further exploration into the movements of this whole system of currents. Their incalculable commercial importance makes such an inquiry any thing but speculative, and should stimulate active and well-conditioned observations. We well know how imperfect the observations by the drift of ships must be; they are rather indications than measurements.

In view of the present state of the case, I would ask attention to the promise of results offered by undertaking an extensive series of current bottle observations; on the line from Cape St. Antonio to Cape Catoche. By systematic proceedings several points might be well illustrated. Suppose a vessel to cross on this line, say twice monthly for a year, throwing over one or two hundred bottles each time, containing slips duly numbered so as to indicate each starting point accurately; these points being regularly distributed on the line run, and checked by the verification of the route sailed. As these bottles proceed on their course, they will become faithful witnesses of the currents, and by their spreading they will show conclusively what the real course of the Gulf Stream is, and whether it is broken, one branch sweeping around the gulf coast, and the other pushing

on northeasterly. With a view to promote their being readily picked up at sea, I would propose that flasks of white glass, blown with broad bases, should be used. These could be seen at a distance, and in a region so crowded with sails as the Gulf of Florida, very many would be picked up while still afloat, thus giving a true measure of mean velocity. A small sailing vessel, such as one of the Key West pilot boats, or the revenue cutter at that station, might, by having a good observer put aboard, make such a course of observations with slight expense in proportion to the results. It is hardly needful that I should here further state the bearings of such a plan, but I think all will concede to it the promise of elucidating some important questions of the Gulf currents. It would surely be much better, could deep sea observations be made also, and to some extent probably they might be connected with a current bottle campaign. The superficial study, ought certainly not to be longer deferred; after this, we can take a next step more wisely.

ART. XXVI.—*Abstract of a Meteorological Journal, kept at Marietta, Ohio:* lat. 39°·25 N. and lon. 4°·28 W. of Washington City; by S. P. HILDRETH, M.D.

MONTHS.	THERMOMETER.							BAROMETER.		
	Mean temperature.	Maximum.	Minimum.	Fair days.	Cloudy days.	Inches of rain and melted snow.	Prevailing Winds.	Maximum.	Minimum.	Range.
January,	40·44	67	22	12	19	1·66	W., S.W., & E.	29·93	28·95	0·98
February,	28·00	60	−5	11	17	3·41	W., N.W.	29·75	28·90	0·85
March,	40·70	75	0	21	10	1·00	W., N.W., & S.	29·63	28·95	0·68
April,	54·70	78	26	12	18	5·00	W., S.W., & E.	29·63	28·70	0·93
May,	60·70	84	42	7	24	12·42	S., S.W., & E.	29·65	28·80	0·85
June,	72·70	99	48	20	10	3·09	S., S.W., & S E.	29·60	29·00	0·60
July,	75·20	95	57	19	12	5·33	S., S.W., & W.	29·63	29·20	0·43
August,	72·13	95	48	19	12	7·42	S., S.W., & W.	29·60	29·15	0·45
September,	64·33	91	38	23	7	1·37	S.W., S., & E.	29·80	29·15	0·65
October,	56·66	85	38	13	18	7·66	S.W., N., & E.	29·80	29·10	0·70
November,	38·80	63	20	3	27	4·82	W., N. W., & S.	29·70	28·85	0·85
December,	40·64	63	14	10	21	8·66	S., S.E., & E.	29·90	29·05	0·85
Year,	53·75			170	195	61·84				

Temperature.—The mean temperature of the year 1858 is 53°·75; more than two degrees above that of 1857, which was 51°·43. The extremes of heat and cold have not been so great as in some years, especially that of cold; the lowest grade of

the thermometer being only five degrees below zero, in February.

Rain and melted snow.—The amount of rain and melted snow is sixty-one inches and eighty-four hundredths, a quantity somewhat exceeding that of any other year since I have kept a register, thirty-two years, and with that kept by Mr. Wood from 1818, making forty years; forty-two inches is the mean amount for a series of years, but in dry periods it sinks sometimes to thirty-two inches, about half that of the past year. The month of May exceeded in quantity that of any other, being nearly twelve and a half inches. It was divided among the seasons as follows: winter $13\frac{73}{100}$ inches, spring $18\frac{42}{100}$ inches, summer $15\frac{84}{100}$ inches, autumn $13\frac{85}{100}$ inches. The quantity of snow was small, four inches being the greatest depth at any one time, not affording sufficient for sleighing.

Winter.—The winter of 1858, was uncommonly mild, the mean being 36°·54; more than six degrees above that of 1857, which was 30°·35; while that of 1856 was 25°·50, the lowest of any one on record. The moderate weather continued until near the middle of February, about which time the Ohio river was open and navigable for steamboats. The mean for December was 41°·20, and that of January 40°·44, being many degrees above that mean for these months, so mild was the weather that it was feared we should have no ice for summer use. The buds of fruit trees swelled as they do in March, and some peach trees on a high sandy ridge of land in Noble county, fifteen or twenty miles north of Marietta, opened their blossoms on the 28th day of January, and what is very curious, notwithstanding the cold in February and March, produced fruit. It was as late as the 18th of February before navigation was closed by ice, and the 24th before the Ohio was frozen over. It remained shut only a few days, and boats were again running by the tenth of March. In a majority of years, the Ohio is closed for a short time in December, but invariably opens again at or near the winter solstice, when there is commonly an abundance of rain. February was a cold month, the mean being 28°·00; whereas in 1857 it was 42°·73, a difference of nearly fifteen degrees. The earlier part of the winter was mild all over the valley of the Ohio.

Spring.—The mean temperature of the spring months is 52°·03; being nearly seven degrees above that of 1857, and a full medium for this climate, the difference being occasioned by the higher temperature of April. The mean of this month is considered as usually representing that of the year, but in 1858 was nearly three degrees above it. The month of May was about the ordinary temperature 60°·70. The early part of March was uncommonly cold, the mercury falling to zero on the seventh of

the month, and for a number of days it was but a little above that point. This severe cold had a disastrous effect on fruit trees of all kinds, especially peaches, apples and pears, the crop of these varieties being entirely destroyed all over the Western States, except in a few favored places, especially in orchards located on the tops of high ridges with a light loamy soil; at this time the blossom buds were red, on the point of opening their flowers. Orchards on islands in the Ohio river were in some measure protected by the proximity of water, and produced a partial crop. The loss to the country must have been more than a million of dollars, as there is scarcely a farmer in the land who has not more or less acres of orcharding, some along the borders of the Ohio raising in ordinary years two or three thousand barrels of apples. Fruit trees blossomed at about their usual period, and when in this state, a severe frost on the 27th of April destroyed the remaining strength of the germs, so entirely that the young fruit all dropped off before it attained the size of a robbin's egg. The grape being later in blossoming, escaped in a measure the ill-effects of frost; but the excessively wet summer rotted and mildewed a large portion of the fruit, disappointing the hopes of the cultivators in affording them only a small crop.

The month of May was excessively wet, raining more or less copiously on seventeen days, summing up at the end of the month the enormous quantity of twelve and a half inches, which is more than all that fell in the spring months of 1857, and a greater amount than ever known before since a register of the rain has been kept. For the three spring months this year the amount was eighteen and a half inches. The effects of this superabundant and constant wet, was very disheartening to the cultivators of the soil. The land could not be plowed at the proper time for the planting of corn and other spring crops, and when it was done, the seed rotted in the earth. Along the margins of the creeks and rivers the bottoms were overflowed, destroying the seeds that had already germinated, and leaving much drift and rubbish, thus marring the grounds for future cultivation. These overflows continued to recur, until as late as the fifteenth of June, and many fields were replanted two or three times, while others were abandoned as hopeless. This excess of rain was not confined to the State of Ohio, but was felt in all the Western States, especially on the river Wabash, where the floods in June were very disastrous. West of the Mississippi river the rains were still more copious, as by a notice in a letter from Lee County, Iowa, there had fallen sixty-five inches from the eighth of April to the first of November, the usual quantity being only forty-four inches; and as December was a very wet month, there was not less than seventy-two inches, or six

feet during the year; an amount usually found only in tropical climates.

Summer.—The mean temperature of the summer months is 73°·34, one degree more than in 1857, and a full average for this region. For the healthy growth of plants there was abundant heat; but the excessive rains so saturated the earth that their roots were in a manner drowned, especially on flat lands, causing a sickly aspect, instead of the usual deep green color seen in common seasons. There was a large proportion of straw in the wheat and oats, but a lack of fullness in the grain; much of the wheat being shriveled and light, a blight or rust having attacked the stems a short time before the harvest, so that in the operation of threshing, a cloud of offensive ill-flavored dust annoyed the workmen, making this labor very irksome. This mould, on the oat crop was still more destructive, causing a total failure in three-fourths of all the fields in the valley of the Ohio; such as escaped were on high grounds and sowed very early. Many fields of wheat were not reaped at all, and left to decay on the grounds, or plowed in for the next crop.

The weather being warm all through the summer and till late in the autumn, gave the Indian corn time to perfect its growth and ripen the grain before the setting in of frosts, thus saving the inhabitants of the west from the disastrous effects of a famine. The grain of this plant, a native of America, is above all others suited to this climate; affording the most nutritious food for man and beast. Potatoes, next to maize as a food for the laboring man, were also a failure. The rot so disastrous in its effects a few years ago to Ireland, destroyed this desirable esculent after it was nearly full grown; and thousands of acres in the southern portions of Ohio, hardly returned the amount of the seed that was planted. Sweet potatoes fared much better, and yielded a fair return to the cultivator. Being a native of a tropical climate, heat and moisture do not injure it if planted on a sandy soil. This year the price of this valuable root was less than that of the common potatoe, when in ordinary years it is double that article. The amount of rain in the summer months was nearly sixteen inches. The maximum heat 99° on the 29th day of June. Although wet summers are accounted to be sickly, yet no epidemic fevers prevailed; it was very healthy.

Autumn.—For the autumnal months the mean temperature is 53°·26, varying but little from that of 1859. During September and October there was no destructive frost, nor until the middle of November, giving late planted corn time to ripen, which it had fully accomplished by the last of October. In ordinary years this crop is ready to be cut by the twentieth of Sep-

tember if planted in due season; all over the uplands of the State it was unusually fine, better on the hills than on the bottoms, as the latter had been too wet for a healthy growth. This abundance of corn furnished the farmers with the means of better fattening their hogs than last year, and the yield of pork is much greater and better in quality. It also bears a fair price, enabling them in some measure, to make up for the loss of their wheat and potatoes. It has been on the whole a disastrous year for the agriculturist, and pecuniary affairs were never more depressed than at present, even more so than in the panic and mercantile failures of 1857, as then he had a fine crop of wheat and abundance of fruit and potatoes to comfort him under his losses.

On the 27th of November there fell seven inches of snow with the temperature at 33°. It rained the following night and in forty-eight hours it was all melted. There has been no ice in the rivers up to this time, 5th of January, 1859, and the greatest degree of cold, the 9th and 10th of December, is 14° above zero.

Floral Calendar.—March 12th, Bluebird seen; 17th, Robbins appear; 20th, Blackbirds in flocks; 25th, Primroses opening, Sugar tree in blow; 29th, Daffodill; 30th, Hepatica triloba, Crown Imperial 12 inches high. April 3d, Crown Imperial open; 4th, Early Hyacinth; 5th, Golden bell or Forsythia virida; 6th, Magnolia conspicua, most of the blossom buds killed in February; 7th, Peach tree, in sheltered localities; 9th, Pear tree opening; 10th, Sanguinaria Canadensis, Pyrus Japonica, a few blossoms, much injured by the cold 23d Feb.; 11th, Pear in bloom; 12th, Spiræa prunifolia, blooms sparingly, much killed by the cold, Double flowering peach, very fine, more hardy than the common; 13th, Cercis Canadensis, or red bud; 14th, Plum and Cherry; 15th, Cucullaria spectabilis; 17th, Apple in full bloom; 18th, Jeffersonia diphylla; 21st, Dwarf Ranunculus Triphyllum uliginosum; 24th, Lilac; 26th, Cornus Florida; 29th, Quercus tinctoria, black oak; 30th, Garden tulip and tree Peony. May 3d, Viburnum dentatum, black Haw; 8th, Prunus scrotina, black Cherry, Cratægus flava, summer Haw, Shell-bark hickory, Aquiliegia Canadensis, Columbine; 9th, Geranium maculatum; 13th, Robinia pseudoacacia, yellow Locust, Dodecatheon Illinoisi, or Prairie Captain, Calceolaria, white and yellow varieties; 13th, Weigelia rosea; 18th, Magnolia tripetala, Castanea equinus, Horse chestnut, Rubus villosus, Blackberry, bore an enormous crop of fruit, more than ever known before; 19th, yellow Harrison rose; 20th, Black mulberry; 22d, Purple peony; 23d, Syringa fragrans. June 2d, Euonymus atropurpurea, wahoos; 3d, Syringa Philadelphica; 4th, Trades-

canthia virginica, Spiderwort; 5th, Rosa Carolina, swamp rose; 8th, Sambucus Canadensis, Elder; 11th, Vitis cordifolia, frost grape; 12th, Rosa multiflora, prariensis; 13th, Triosteum perfoliatum, feverwort, Rhus typhina, sumach; 25th, Wheat harvest begins in warm exposures; 28th, Asclepias cornuti, milkweed. July 6th, Lobelia spicata; 15th, Rhus radicans, trumpet creeper; 16th, June-eating apple ripe; 17th, Sweet bough apple; 18th, Blackberry ripe; 26th, Cassia Marylandica, wild senna, in flower. August 11th, Watermelon ripe in open fields; 19th, Sweet potatoe in market, good size.

Marietta, Ohio, January 5th, 1859.

ART. XXVII.—*Remarks on the Lower Cretaceous beds of Kansas and Nebraska;* by F. B. MEEK and F. V. HAYDEN.

(Extracted from the Proceedings Acad. Nat. Sci., Philad., Dec., 1858, with additions by the authors.)

THE Cretaceous System as developed in Nebraska, is clearly divisible into five distinct formations, which have, for convenience, been numbered 1, 2, 3, &c., from the base upwards. Although at first entertaining some doubts as to whether No. 1, or the lowest formation, might not be older than Cretaceous, we always placed it provisionally, in our published sections, in the Cretaceous system. More recently, after a careful review of the subject, we became satisfied from the modern affinities of numerous dicotyledonous leaves found in this formation, that we hazarded little in regarding it as a settled question that it could not be older than Cretaceous, and so expressed ourselves in our paper read before the Academy of Natural Science, Philadelphia, March, 1858.

The references of this formation to the Cretaceous, however, was not without some exceptions generally admitted, for Professor Jules Marcou, in his work on the "Geology of North America," page 143, refers it to the New Red Sandstone, and in a subsequent publication* he places it in the Jurassic; while some investigators in this country also, inclined to the opinion that it must be Triassic. In the midst of these conflicting opinions, although satisfied we were right, we wished, in order to remove all doubts from the minds of others, to have the opinion of some good authority in fossil botany, (a department of palæontology to which we have given little attention,) respecting the fossil leaves on which we mainly based our views in regard to the age

* Notes pour servir a une description geologique des Montagnes Rocheuses, p. 20.

of this formation. Consequently, we sent outline sketches of a few of them to Professor Oswald Heer,* the distinguished authority in fossil botany at Zurich, Switzerland, informing him they were from a formation we regarded as Cretaceous, and requesting him to let us know to what genera and geological epoch he would refer them. This letter was sent to Professor Heer in August last, before we started to Kansas, and on our return, in the latter part of October, we were disappointed at finding no reply from him. After waiting some days longer, and receiving no answer from Professor Heer, we concluded our letter had either failed to reach him, or that he was unwilling to express an opinion based upon mere sketches of the leaves; consequently we submitted the whole to Dr. Newberry, who had then returned to Washington, and in whose opinions on this subject we have the fullest confidence.

After examining the specimens, Dr. Newberry gave us a written statement bearing date, Nov. 12, containing a list of the genera to which he had referred the leaves, together with some interesting remarks and generalizations, in which he expressed the opinion that they are certainly Cretaceous, some of them belonging to genera peculiar to that epoch, and that the whole belong to more highly organized plants than are known in the Triassic or Jurassic flora.

Knowing as we did that the rock from which these plants were obtained, beyond all doubt, holds a position beneath, at least, eight hundred feet of Cretaceous strata, containing great numbers of *Ammonites*, *Scpahites*, *Baculites*, &c., it of course never once occurred to us that any person might suppose it Tertiary.

About the thirteenth of November we sent on to the American Journal of Science, a communication containing Dr. Newberry's list of the genera to which he had referred these plants, with some extracts from his remarks, all of which appeared in the January number of that Journal.† Some two or three weeks after we had corrected the last proof of this paper, we received (13th of Dec.) a letter from Professor Heer, bearing date of Nov. 26, in which he informed us that our letter had reached him at a late date, in consequence of his absence from home, and that after his return, other engagements had prevented him from replying sooner. In this letter Professor Heer, in accordance with our request, sent us a list of the genera, as near as it was possible for him to make them out from hastily drawn sketches, and also kindly furnished brief diagnoses of the species, stating at the same time that although one of the outlines resembles a

* Our friend Dr. Newberry was then in New Mexico.

† These were published in the last number of this Journal.

Cretaceous genus (Credneria,) the nervation being obscure, and the others more like Tertiary forms than anything known in the Cretaceous of the old world, he was inclined to the opinion that they are Tertiary.

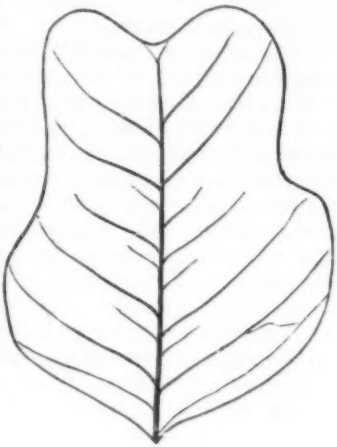
Along with Professor Heer's letter, we also received a printed pamphlet, entitled "*Letters on some points of the Geology of Texas, New Mexico, Kansas and Nebraska; addressed to Messrs. F. B. Meek and F. V. Hayden, by Jules Marcou.*" In this pamphlet Professor Marcou quotes Professor Heer's conclusions in regard to our fossil plants, and expresses the opinion that No. 1, of the Nebraska section, is both Miocene and Jurassic, or in other words, that we have included in it strata belonging to each of these two widely different geological epochs.

Having a very high regard for Professor Heer's opinions on any question in fossil botany, where he has had an opportunity to examine the specimens themselves, or to study good figures and descriptions, we are quite sure, had the whole collection been submitted to him, instead of mere sketches of a few of the species, his opinion would have been very different. At any rate we can assert, with the fullest confidence, that it is absolutely *impossible* that this formation, or any part of it, can be Tertiary, for we know it passes, as already stated, beneath at least eight hundred feet of Cretaceous strata. This is not mere conjecture, nor an inference drawn from having seen this formation under circumstances leading us to *suppose* from the dip of the strata, that it must pass beneath the Cretaceous if continued in a given direction at the same angle of inclination, but from the fact that it has actually been seen, directly beneath the other Cretaceous rocks, not merely at one place, and by one observer, but by several persons at numerous localities.

In order to satisfy others that we are not mistaken in this, we will give a few of the many facts in our possession, bearing on this question. In the first place, we would remark that the farthest point towards the south at which we have seen this formation, is near Smoky Hill river, in Kansas, latitude 38° 30′ north, and longitude 97° 30′ west. Here we found it forming the upper part of several isolated elevations known as the "Smoky Hills," at an altitude of about 1200 feet above the Missouri at Fort Leavenworth. At this locality, however, we saw no rocks overlying it, and consequently have no *stratigraphical* evidence that it is the same rock seen by us at other localities under Cretaceous beds; but our lithological and palæontological evidence is quite conclusive on this point, for this rock in color, composition, and all other respects, is undistinguishable from No. 1, of the Nebraska section, as seen near the mouth of Big Sioux river on the Missouri, and contains numerous fossil leaves, some of which are identical with those occurring in No. 1, at the last

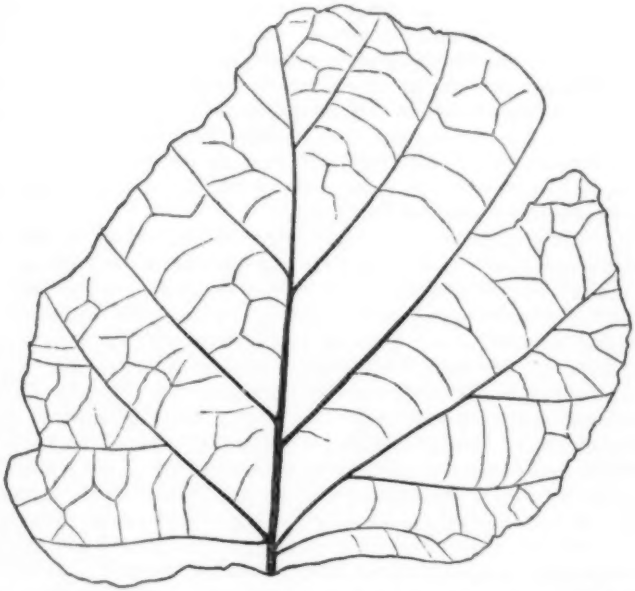
mentioned localities. Amongst these leaves Dr. Newberry has also identified at least one genus (*Ettingshausinia*, No. 3 of the following cuts,) peculiar to the Cretaceous System. The specimens from which this outline was drawn, was not in our possession at the time we sent the outlines to Professor Heer, but was afterwards found associated with several of the species from which the sketches sent him were drawn. The annexed cuts, Nos. 1 and 2, represent other forms from the same rock.

1.



Bearing in mind that all the rocks here have a gentle but uniform inclination or dip to the northwest; and that the formation under consideration con-

2.



sists of red, yellowish, and other colored sandstones and clays, with generally more or less impure lignite and ferruginous concretions, we will be prepared to recognize it at lower and lower elevations as we proceed northward.

8.

Without undertaking to mention in detail all the intermediate places where this rock is known to occur, we pass at once to localities where it has been observed by various persons *beneath* Cretaceous beds. First at several points on the Republican fork of Kansas river, about eighty miles above its mouth, and some sixty miles nearly due north of the Smoky Hills, it was seen by Dr. H. Englemann at an elevation of about 1000 feet above the Missouri, at Fort Leavenworth. Here Dr. Englemann describes it as "a coarse grained, friable dark brown ferruginous sandstone, and very compact white and light colored shaly sandstone."

Near this locality he saw it *overlaid* by Cretaceous beds, the section given by him being as follows descending. "The top was formed of a white granular limestone, and another more impure yellowish variety *full of Inoceramus.* Below there seemed to be a sandy clay, probably from decomposition of arenaceous and argillaceous slates, and then a stratum of gray compact subcrystalline limestone in thin layers, *full of Inoceramus Cripsii.*

In the lower part of the hill the ferruginous sandstone was exposed." Report Sec'y War, Dec. 5, 1857, p. 497.

Again it has been seen by several observers at different times near Little Blue river, not far from the Kansas and Nebraska line,—lat. 40° and a little west of the 97° of west longitude. Here at an elevation of about 700 feet above the Missouri at Leavenworth, or three hundred feet below the horizon of the localities on Republican fork, and, five hundred feet below the elevation of the outcrops seen by us on the summits of the Smoky Hills, our deceased friend, Mr. Henry Pratthen, saw near Wyeth's creek, in 1853, the following exposures in descending order:

1. Slope, height not given.	
2. Yellow and whitish limestone filled with casts of *Inoceramus*, referred by him to *I. myteloides* = *I. problematicus*.	No. 3, Nebraska Sec.
3. Slope, thickness not given.	No. 2, Nebraska Sec.
4. Red ferruginous sandstone *with leaves of dicotyledonous tress.*	No. 1, Nebraska Sec.

A short distance west of this exposure Dr. J. G. Cooper informs us he saw outcrops of a red sandstone in the valleys at about the same elevation; and above this, exposures of dark gray laminated clay answering exactly to the description of No. 2, of the Nebraska section, while above the latter, near the tops of the hills, he met with outcrops of *light colored limestone containing numerous casts of Inoceramus.*

At other localities not far to the southwest of the foregoing, Mr. Hawn saw exposures of *light colored limestone* forty-five feet in thickness, *containing great numbers of Inoceramus* which we referred, from specimens sent by him, to *I. problematicus.** Below this there was a slope of twenty-seven feet in which he saw no exposures, while still lower he observed outcrops of *dark ferruginous and yellow sandstone, and various colored clays with impressions of leaves*, resembling, as he supposed, those of oaks and willows. (See his section published by us in the Proceedings of the Academy of Natural Sciences of Philadelphia, May, 1857.)

Proceeding northward from the last mentioned localities, we find on reaching the Loup fork of Platte river, near the eastern limits of the Pawnee reservation, outcrops of the light colored *Inoceramus* beds already mentioned, (No. 3, Nebraska section,) near the water's edge; and at the mouth of Loup fork, on the

* We have referred this species to *I. problematicus* with some doubt; it is the same species inscribed by Dr. Schiel in the second volume of the Pacific Rail Road Report, page 108, plate 3, figure 8. It is rather longer on the hinge than is common in *I. problematicus*, from which it may be distinct. We always refer to this shell in speaking of *I. problematicus*, in this paper.

Platte, the red sandstone No. 1, so often referred to, crops out near the river margin, *while the Inoceramus beds are seen in the bluffs above it.* Going down the Platte in a direction nearly contrary to the dip of the strata, we find this sandstone rising up so as to form near the mouth of Elk Horn river, bluffs some sixty feet in height. Here it seems to rest directly upon upper Carboniferous rocks. Continuing on down the Platte, we find this red and yellow sandstone rising higher and higher in the hills until we come within five or six miles of the Missouri, where it is seen with its base elevated near sixty feet above the Platte; and there are probably outliers of it between that point and the Missouri at greater elevations. So that we find the same formation which at the Smoky Hills, is elevated twelve hundred feet, —on the Republican river, one thousand feet,—and near Little Blue river seven hundred feet, above the Missouri at Leavenworth, has by the gradual northwestern dip of the strata, sunk to within about one hundred feet of the Missouri at the mouth of the Platte.*

Ascending the Missouri from the localities just mentioned, we see occasional exposures of the upper Carboniferous rocks, which gradually sink lower and lower until they pass beneath the river near Florence, to be succeeded by the reddish and yellow sandstones, &c., of No. 1.—(Nebraska section.) At localities along the river above this, occasional exposures of this formation are seen with its characteristic fossil leaves; and at several points, some thirty miles below the mouth of Big Sioux river, it forms perpendicular escarpments of yellowish sandstone rising from the water's edge to an elevation of about eighty feet; while at higher points, back on the summits of the hills, *the same calcareous beds are seen, containing Inoceramus problematicus.* Here at a quarry in the sandstone (formation No. 1,) some twenty feet above the level of the river, one of us (Dr. H.) collected a large number of fossil leaves, some of which are identical with species found by us in this rock at the Smoky Hill locality already mentioned. The sketches of leaves sent by us to Prof. Heer were mostly drawn from specimens collected at this locality.

At the mouth of Big Sioux river a low bluff of this formation, not more than fifteen or twenty feet in height, is seen, and on the hills back a little from the river, at a higher elevation, the same *Inoceramus* bed crops out at several places, and is used for

* The gradual descent of the Missouri river makes its surface at Fort Leavenworth, about three hundred feet lower than at the mouth of the Platte, hence the exposures of No. 1, seen at the latter locality, near one hundred feet above the Missouri, are some four hundred feet above the level of the Missouri at Fort Leavenworth, and of course about three hundred feet lower than the Little Blue river outcrops. The dip however, is greater than this would indicate, for the strata incline towards the northwest, while the mouth of Platte river, is northeast of the Blue river localities.

making lime. At another locality about eight or ten miles up the Big Sioux river, which comes in from the northwest, one of us (Dr. H.) saw No. 1, containing its characteristic fossil leaves, *directly beneath* No. 2, of the Nebraska section. The exposure presented the following beds in the descending order:

1. 20 feet exposed of light gray limestone and marl, containing *Inoceramus problematicus.*	No. 3, Nebraska Sec.
2. 45 feet dark laminated clay with ferruginous concretions containing fish scales.	No. 2, Nebraska Sec.
3. 15 feet exposed above the edge of the water, consisting of yellowish friable sandstone, with a thin bed of impure lignite above, and some layers of various colored clay below, containing *dicotyledonous leaves.*	No. 1, Nebraska Sec.

One of the sketches of a long lanceolate leaf, like some of the existing species of Salix, sent by us to Prof. Heer, was drawn from a specimen collected from one of the lower sandstones here.

Again at another locality on the Missouri, about thirty miles above the mouth of Big Sioux river, No. 1, was seen by one of us (Dr. H.) only five feet above the water's edge, and *immediately overlaid* by No. 2, of the Nebraska section, containing its characteristic species of *Ammonites;* and directly over the latter, he saw No. 3, containing *Inoceramus problematicus.** At this locality he also found in No. 1 some of the same fossil leaves that characterize it at the other places already mentioned.

On ascending the Missouri, above the last named locality, formations No. 2, 3, 4 and 5 are seen to sink at the same gradual uniform rate of dip, in regular succession, beneath the level of the river; so that on reaching Heart river, we find the top of No. 5 nearly down on a level with the water's edge, and a short distance above that locality, it passes out of sight to be succeeded by the Great Tertiary Lignite basin of the upper Missouri, which overlaps it on the hills along the river for some distance below.

From the foregoing statement, we think it will be clearly understood, that formation No. 1 of the Nebraska section holds a position *beneath* the other cretaceous deposits of that region; while the occurrence in it of the remains of highly organized angiosperm dicotyledonous plants proves that it cannot be older than Cretaceous. It may be argued, however, that it may in part be Cretaceous and part Tertiary, or at any rate that *some* of these leaves may have been obtained from overlying Tertiary beds which we have confounded with the Cretaceous below.

* It is of course unnecessary for us to inform geological readers that a rock overlaid by strata containing *Ammonites* and *Inoceramus*, cannot be Tertiary because these genera became extinct at the dawn of the Tertiary epoch.

This, however, is impossible, simply because specimens of nearly all the species found at the various localities have been quarried from the same bed at Blackbird Hill, and the whole,—not a part only—of this formation, passes beneath all the other Cretaceous rocks of the northwest. In addition to this, we have extensive collections of plants from the Tertiary of Nebraska, not a single species of which is identical with those from No. 1.

When we stated in some of our papers that it was possible we might have included in this formation beds not belonging to the Cretaceous, it was not because we thought any part of it might be Tertiary, but because we suspected some of the lower beds referred to it in Kansas might possibly be Jurassic; and we are even now prepared to believe that it may yet be found to repose on Jurassic rocks in that Territory, as it does at the Black Hills, in Nebraska.

Art. XXVIII.—*Geographical Notices.* No. VI.

Semenow's Explorations in Central Asia, 1857.—The trigonometrical survey of India, the exploring expedition of the Brothers Schlagintweit, and the researches of Russian travellers are making valuable contributions to our knowledge of Central Asia. In a recent number of Petermann's journal a highly important paper is presented in respect to the expedition of P. V. Semenow, in the neighborhood of the Balkasch lake. The article is based upon original data furnished by the traveller, and dated St. Petersburg, June, 1858.

We condense and translate such portions of the article as are most interesting, regretting that we are not able to transfer it entire.

To the south of the Russo-Siberian frontier and military post route, which follows the course of the Irtysh, there extends a wide and sterile tract, universally known as the Kirghese Steppe. This Steppe has a rocky substratum mostly crystalline, but in part also sedimentary, swelling into hills and occasionally aggregated into small and low mountain-clusters. Its characteristic features are aridity, the absence of trees, scarcity of streams, relatively insignificant height of the hills which hardly in a single instance deserve the name of mountains, and numerous salt-deserts with their accompanying halophytes (salt water plants).

But as we arrive at the river Ajagus, an eastern tributary of the Balkhash Lake, and pass beyond the low sandy downs, that by their depression, their saline character, and their numerous standing pools bordered with sedges, indicate a former connection between the Balkhash and the Ala-kul, we enter an entirely

different region. This long low mostly dry bed of what must formerly have been one continuous lake extending from the 74th to the 82d meridian east of Greenwich, forms a marked dividing line between the mountain-systems and general physical features of Central Asia on the south, and Siberia on the north. From this line onward we encounter a continuous series of magnificent Alp-lands, rising height above height till they at last blend with the Thian-schan, the most central of the mountain ranges of Asia. This line too forms a natural limit, north of which we no longer find many of the vegetable growths or the animals of Central Asia, such for instance as the *Populus diversifolia* and the *Pyrus Sieversiana* among trees, or the tiger, the hedgehog, the pheasant, &c., among animals.

Semenow's explorations, made in the year 1857, relate mostly to this Central Asiatic Alp-land, which though comparatively small in extent, is yet charming for the variety of its scenery, and attractive to the physical geographer for its union of so many different zones with their diverse characteristics of soil, of temperature, of vegetable and animal species, etc. It is bounded on the north as we have stated by the Balkasch-Ala-kul Lake belt, on the east by the snow-covered crest of the Dzungarisch Ala-tau, and on the south by the perpetual snows and glaciers of the Thian-schan, and it comprises all altitudes between the wide extremes of 600 and 2000 Paris feet.* This region, in addition to its interest for natural science, is also attractive to the ethnographer, as having been from the earliest times one of the most important stations for those vast wandering hordes which have successively overrun Europe. For here in the broad and fertile valley of the Ili they would often stop for several years, and then with the fresh strength and energy gained by their repose, take up their march around the southern shore of the Balkhasch, either northwest toward Europe, or southwest towards Turan, Southern and Western Asia.

The Ili divides this portion of Central Asia into two parts, the northern called "the land of seven rivers," the southern "the land across the Ili," (Transilian,) names given them by the early Russian settlers.

The distinctive features of this region are its three lofty Alp-lands, viz.:

1. The Dzungarisch Ala-tau, (closely connected with the Talki-chain that divides the Ala-kul and the Ili valleys,) with a medium ridge altitude of 6,000 feet, and a peak altitude of 12,000 feet.

2. "The Ala-tau across the Ili," between the Ili valley and the Issyk-kul plateau, with a medium ridge altitude of 8000 feet, and a peak altitude of not far from 14,000 to 15,000 feet; and

* Semenow's measurements are all given in Paris feet.

3. The Thian-schan, between the Issyk-kul plateau and the plains of Little Bucharest, with a medium ridge altitude of about 11,000 feet, and a peak altitude of perhaps 20,000 feet. The Dzungarish Ala-tau on its western slope and the Transilian Ala-tau on its northern face decline directly into a broad Steppe-level that stretches away at the varying elevation of 1500 to 500 feet to the Balkasch basin, and comprises the entire western and northwestern portion of this region. And the nearer we approach to the Balkasch, the more flat, arid, unfruitful, sandy and saline does the soil appear, gradually becoming covered with the *Haloxylon ammodendron* and with halophytes; and the streams, which as they issued from the mountains were bright, clear and rapid, become more sluggish and turbid till at last they come to a stand amidst sandy downs and sedgy marshes, and but three of them, viz.: the Lepsa, the Karatal, and the Ili actually reach the Balkasch Lake.

But the transition zone between the mountain-land and this Steppe, is on the contrary, one of the finest agricultural tracts on the continent. It possesses a deep vegetable mould, a luxurious growth and such an abundance of water that the inhabitants, the Kirghese, the Buruti and the Russian Cossacks, apply an artificial irrigation to their fields with surprising facility.

If we now take a general survey of this region in its practical adaptations to the wants of its inhabitants we shall find that it includes four natural zones, each offering its peculiar tribute to the general welfare. 1. The Steppe-zone, 500 to 1500 and in some places 2,000 feet above sea-level, affords most excellent winter quarters for the Nomads, on account of its mild climate and its almost entire exemption from snow. 2. The agricultural zone, from 1,500 to 4,000 feet of altitude, contains rich arable lands. 3. The pine-tree zone, from 4,000 to 7,000 feet in altitude, yields abundance of timber wood for building; and 4. the Alpine meadow zone, from 7,600 to 9,000 feet in altitude, entices the Nomads by its wholesome air and its rich pasturage to resort thither during the summer. There are two other zones, viz.: the High-Alp and glacier-zone from 9,000 to 11,200 feet in altitude, with its brilliant flora, and the snow-zone, or the region of perpetual snow; but these will forever remain practically of no immediate importance to colonists.

We omit Semenow's measurements and notes, geological, physical and enthusiastically descriptive, taken during his explorations in the Dzungarisch Ala-tau and the Thian Schan, and give briefly the results of his observations upon the Issyk-kul lake. Between the Transilian Ala-tau and the Thian-schan ranges there is a plateau of 4,200 feet altitude, 230 wersts in length, and 70 in breadth, in which the charming lake of Issyk-kul is situated. This lake is 150 wersts in its extreme length by

50 wersts in breadth. Its waters are brackish and unpalatable. It is fed by over 40 short mountain streams, which fertilize the otherwise sterile soil of the plateau, and which are fringed by long lines of trees. But little sedge is to be met with, and that only around the indentations of the lake. On the contrary, the *Hippophae rhamnoides* forms a thick bushy growth in the neighborhood of the shore. Between the mountains and the lake there is a belt of from 7 to 20 wersts in breadth. At one point only do we find the case otherwise, viz.: the Kesse Tsengyr on the northern shore, where a spur from the Ala-tau approaches so near the water that there is merely room enough left for a waggon road. The immediate border of the lake is in general low and sandy; but around some of the bights the land is more elevated and presents a steep descent to the water; in such places the beating of the waves frequently wears away the loose alluvial strata of which these bluffs consist, so that large masses will at intervals crumble into the lake. Semenow saw no islands in the Issyk-kul. From the fact that so many streams flow into it, he conjectured that it must at some point effect an outlet. Geographers had hitherto represented the river Tschu as such an outlet, but Semenow followed the Tschu up toward its source and ascertained that it approached the lake no nearer than 5 wersts. Here it breaks through a frightful gorge in the Byam mountain, a continuation of the Transilian Ala-tau, and flows N.E. to unite with the Kebin. If the waters of the Issyk-kul had at some previous period been about two hundred feet higher than they now are, (and there are water-marks near the base of the mountain which may warrant such a supposition,) then the lake may itself have opened the Byam gorge, and so discharged its surplus flood into the Tschu. Such a former high state of its waters will account for the frequent conglomerate strata on its banks, which were doubtless formed in the lake-bed, and brought to view on the recession of the waters. Semenow found moreover an additional argument for this hypothesis in a legend of the Buruti that the ruins of a submerged city are at certain seasons visible at the mouth of the Tub, and under the surface of the water. In fine his explorations led him to conclude that if it were not for the existence of the Byam gorge, there is no good reason why the Issyk-kul might not rise not merely 200 feet but many hundred feet above its present level.

It only remains for us now to speak of the temperature of the Issyk-kul plateau. Near Viernoie, the second Russian settlement on the military road to the Transilian Ala-tau, and directly to the north of these mountains, the ground is commonly covered with snow only in January and February. In the Issyk-kul plateau snow lies on the ground for over four months of the winter season. In the beginning of May when at Viernoie,

apricot and apple trees are in blossom, at Issyk-kul it freezes after sundown, and the use of furs is indispensable. Still we are surprised to learn that notwithstanding the elevation of the Issyk-kul it is never frozen, although some of its smaller bays are sometimes encrusted with thick ice. This fact is doubtless to be attributed to the temperature of the deep bed of the lake.

The Glaciers of the Tengri-Tagh.—The Tengri-Tagh, a colossal mountain range directly to the east of the Thian-schan, and whose loftiest peak, the Tengri-Khan, soars to a height, as estimated by Semenow, of 20,000 feet, exhibits on its northern declivities a series of glacier formations that are not surpassed in their dimensions by those of the Alps. Semenow informs us that he had hitherto doubted whether true Alpine glaciers could exist in such a dry climate as that of Central Asia. But the most essential features of their formation, viz.: an enormous accumulation of perpetual snow, and basin-like depressions towards the inner termini of the high mountain valleys, were here, and he could doubt no longer when his further explorations were at last "rewarded with the view of three Alpine glaciers and a vast glacier sea." "The valleys into which the glaciers of the Tengri flow are so flat and broad, and their slope is so inconsiderable, that the plastic ice can more readily expand on all sides into glacier seas, than move forward and downward." The Tengri glaciers differ from the Alpine in two respects, viz.: in the absolute level of their upper and lower termini, and in their color. The latter are sometimes found to reach a point 5,500 feet below the snow-line, while 2000 feet appears to be the utmost vertical extent of the former. And may not this lesser range of absolute level in the Tengri glaciers, joined to their lesser slope, permit the snow-light the more readily to stream through them, thus accounting for their water-green color, so different from the delicate blue that marks the glaciers of the Alps?

Trigonometrical Survey of India. Measurement of Himalayan Peaks.—In the last number of this Journal, it was mentioned that a third peak higher than the once pre-ëminent Dhaulagiri had been measured near the Karakorum pass in the Kuen Luen Mountains. The information was communicated to the Royal Geographical Society of London, Nov. 23, 1857, and briefly reported in the Athenæum. The measurement (giving a height of 27,928 feet) was made by Messrs. Montgomerie and Elliott Brownlow under the direction of Col. Waugh.

Various inquiries having been made of us in respect to the measurement of the peaks in Central Asia, we give in this connection the data reported by Col. Waugh, to the East India

Company, concerning the measurement of the four famous peaks of the Himalayas.*

Name of the Peak.	Station of Observation.	North latitude.			Longitude East from Greenwich.			Height above sea level in English feet.
Choomalari or I.	Senchal, H. S.† ..	27°	49′	41,5″	89°	18′	43,1″	23,946
	Tonglo, H. S.	"	"	41,5	"	"	43,1	" 41
	Mean,..	27	49	41,5	89	18	43,1	23,946
Kinchinjunga or IX.	Doom Dangi, T. S.	27	42	9,5	88	11	26,4	28,151
	Senchal, H. S. ...	"	"	9,3	"	"	26,2	" 50
	Birch, H. S.	"	"	9,4	"	"	26,2	" 63
	Thakoorganj, T. S.	"	"	9,3	"	"	26,7	" 47
	Tonglo, H. S.	"	"	9,3	"	"	26,2	" 80
	Bunderjoola, T. S.	"	"	9,2	"	"	26,1	" 42
	Menai, T. S.	"	"	9,2	"	"	26,3	" 72
	Baisi, T. S.	"	"	9,6	"	"	26,3	" 60
	Harpoor, H. S. ...	"	"	9,5	"	"	26,3	" 40
	Mean,..	27	42	9,4	88	11	26,3	28,156
Mount Everest or XV.	Doom Dangi, T. S.	27	59	16,5	86	58	5,8	—
	Menai, T. S.	"	"	17,1	"	"	6,1	28,990
	Harpoor, T. S. ...	"	"	16,5	"	"	5,7	9,026
	Ladnia, T. S.....	"	"	16,7	"	"	5,8	8,999
	Janjpati, T. S.....	"	"	16,7	"	"	6,0	9,002
	Miriapoor, T. S. ..	"	"	17,0	"	"	5,8	9,005
	Lirol, T. S.	"	"	16,7	"	"	5,8	8,992
	Mean,..	27	59	16,7	86	58	5,9	29,002
Dhaulagiri or XLII.	Ramnagar, T. S...	28	41	47,9	83	32	8,8	—
	Morairi, T. S.	"	"	48,1	"	"	8,3	26,815
	Banarsi, T. S.	"	"	48,1	"	"	8,7	" 00
	Slaonbarsa, T. S. .	"	"	47,8	"	"	8,9	" 60
	Poovenah, T. S. ..	"	"	47,8	"	"	8,9	" 43
	Ghaos, T. S.	"	"	48,2	"	"	8,2	" 06
	Toolsipoor, T. S...	"	"	48,2	"	"	8,4	" 01
	Anarkali, T. S. ...	"	"	47,8	"	"	8,8	" 00
	Mean,..	28	41	48,0	83	32	8,6	26,826

We also give the following general account of the progress of the Trigonometrical Survey of India, from the annual address of Sir R. I. Murchison, in 1858, as president of the Royal Geographical Society.

"The trigonometrical survey of India was commenced by Colonel Lambton in 1803, and continued by him till his death in January, 1823. During that period he measured an arc of

* v. Peterm. Mittheil. 1856, p. 380.

† H. S. signifies Hill Station, and T. S. Tower Station.

the meridian from Punnae in 8° 9′ 35″ near Cape Comorin to Damargidda in lat. 18° 3′ 16″, being about ten degrees of latitude, and extended a net of triangles over the south part of the peninsula of India, reaching on the east side of the principal meridian to the 19th parallel. Colonel Everest, who had been his chief assistant since 1817, and succeeded him at his death, completed the section commenced by Lambton, and extended the arc to Seronj, lat. 24°, near which place he measured a base of verification. This is the most important base in the trigonometrical survey of India, as all the work to the north, east, and west is dependent upon it. Colonel Everest carried on the measurement of the meridional arc to its completion in the Dehra Dún, lat. 30° 19′; the whole extent from Cape Comorin being 22½° of latitude. He also extended a longitudinal series from the Seronj base to Calcutta, in the neighborhood of which he measured a base of verification. From points selected on this series originate distinct sects of meridional series, the northern limits of which are united by a longitudinal series running along the foot of the great mountain chain, which thus completes the triangulation of that vast tract, comprising about 223,000 square miles.

"When this distinguished officer left India, Colonel, then Captain Waugh, who had been his chief assistant since 1832, was appointed his successor in December, 1843, and following up the admirable plan of survey laid down by his predecessor, the principles and methods of which have been described by Everest,* he worked out the several series left unfinished between the meridional arc and that of Calcutta. Finally he measured a base of verification at Sonakoda, lat. 25° 18′, long. 88° 18′, and also completed the triangulation of the south coast series from Calcutta to Ganjam.

"Colonel Waugh then commenced operations on the west of the great meridional arc, and measured a longitudinal series from the base of Seronj, passing through Rajputana and the sandy desert to Karachi, upwards of 700 miles in extent, where a base of verification was measured, whilst the triangulation of the Bombay meridian was connected with this series. He further extended another series in a northwest direction from the stations of the meridional arc, Banog and Amsot, through the plains of the Panjab and a great portion of the mountainous tract to Peshawar. Again, a base of verification was measured near Attock, the series embracing an area of about 67,000 square miles. A meridional series is far advanced from the base at Karachi, along the Indus, to that near Attock. This operation will complete a gigantic geodetical quadrilateral, of

* Account of the Measurement of the Arc of India, 2 vols., 4to, 1847.

which the great arc series forms the eastern side. Simultaneously with these trigonometrical operations, most minute and elaborate topographical surveys have been executed under the superintendence of Colonel Waugh throughout the greater portion of these tracts.

"Lastly, having determined that of all the mountains whence the affluents of the Ganges run, the loftiest summit is situated about midway along the Himalayan chain, and finding that this culminating point (N. lat. 27° 56′, E. long. 86° 53′) was 29,002 English feet above the sea, and consequently 846 feet loftier than the famous Kinchinjunga of Nipal, Colonel Waugh has gratefully and appropriately named this, the highest known elevation in the world, Mount Everest, after his valued geographical instructor."

Central Africa. Expeditions of Capt. Burton and Dr. Roscher.—Our readers are already acquainted with the expedition of Capt. Burton, and of his determination to reach, if possible, the mysterious lake of Central Africa, by penetrating westward from the coast of Zanzibar. Letters received from Rev. J. Rebmann, missionary at Zanzibar, by the Church Miss. Soc. of London, and published in their Record for December last, state that Capt. Burton had reached the lake Uniamesi, but give no further particulars in respect to his journey. Three letters from the explorer himself giving his observations in Zanzibar, written in a spirited style, are given in Blackwood's Magazine for February, March and May, 1858.

The missionary letters just alluded to, announce also the arrival in Zanzibar of the German traveler, Dr. Albrecht Roscher, and of his departure for the reported snow peaks near the equator. Dr. Roscher is represented as qualified in every way, by his previous studies, his energy of character and his excellent outfit, to undertake this difficult exploration. He proceeds under the patronage of the king of Bavaria. From his efforts and those of Burton, we have good reason to expect that two of the great African problems will be solved, the extent of the great lake, and the height of the equatorial mountains. There is reason to hope that light may also be thrown on the older problem of the sources of the Nile.

From a recent number of Petermann's Journal (1858, p. 344) we have translated the following passages which occur in a paper by Dr. Roscher, showing the reasons for his selection of the coast of Zanzibar as a point of departure for Central Africa.

In projecting the plan of an exploring expedition into Central Africa we must depend mainly upon the history of travels in those parts: for these will both suggest what is still to be accomplished and instruct us as to the means to be employed and the course to be pursued. A due regard to the experience

of earlier travelers will alone enable us to discover new routes into unexplored regions, upon which we need not expect to meet with those insurmountable barriers that have presented themselves to our predecessors; and yet the ready abandoning of expectations, prematurely indulged in respecting earlier and more recent expeditions, is a source of greater surprise than the fact that so large a portion of Africa remains unexplored.

All the accounts of travels, which have aided us in the construction of the map of Africa, do not give us any information relative to the central and northern portions of the interior of Africa, although these regions contain the solution of the most important geographical questions, and furnish the key to a proper conception of the physical features of the continent. The remarkable travels of Livingstone prove that an expedition into South Africa is attended with difficulties comparatively small, and that the failure to advance farther into the interior must be attributed, not to the hostility of the natives and the unhealthy climate, but mainly to the selection of an unfavorable starting-point. The course which has thus far been most frequently pursued, that of approaching the interior from the north, is the least practicable. The traveller meets with obstacles at the very boundary which separates the Mohammedan and heathen tribes; for among the former, fanaticism and avarice, among the latter, the fears of slavery, prevail, and every one advancing from the camp of the enemy is regarded as a spy. These insurmountable barriers have existed since the occupation of Africa by the Mohammedans, and here it was that the Arabian geographers, in their additions to the geography of Ptolemy, imagined the snow-covered Mountains of the Moon to be situated.

The Nile-expedition was thus prevented from penetrating any farther than the breadth of the river afforded them a protection against the assaults of the natives, which rendered it impossible for them to reach the source of the Nile. Dr. Barth also was convinced that an advance into South Africa from Lake Tsad was impossible, and hence, contrary to all instructions, he directed his course westward so that the original object of the expedition was not attained. Dr. Vogel was next sent out, and strong hopes were entertained that he might meet d'Escayrac at the source of the Nile, although even at that time it might have been proved that this point was the last one at which the two expeditions would be at all likely to meet. Dr. Vogel's journey to Waday furnishes the strongest proof that he, like his predecessors, was convinced that the so-called Mountains of the Moon, even if in reality no mountains, yet constituted an insurmountable barrier.

The time has certainly come when merely fruitless attempts must be done away with, and when travellers into Southern

Africa ought to take their point of departure in South Africa. There is no means of direct communication between any point on the western coast and Europe or with the Interior, and hence none is suitable for the fitting out of an expedition. The true condition of affairs in this region, may be best inferred from the opinion of Galton, who assumed that a traveller could advance into Africa only very gradually. Finding the arrangements previously made not suitable to the climate of the regions to be traversed, the traveller would be forced to return for the purpose of entering upon new preparations.

The expedition of Caillié, Bruce and Livingstone, prove how erroneous this assumption, and yet it is applicable to such travellers as seek to advance from the western coast, and by reason of the imperfect means of communication, daily meet with difficulties of which they could not have had any previous knowledge. Besides, the seaboard towns are, as a general thing, very unhealthy, and the fever is specially severe upon those newly arrived; so that the explorer on returning need not expect to experience a relief from his toils, but only new dangers.

The successful expedition of Dr. Livingstone has turned the attention of all to South Africa. The point of departure in his expedition, was Lake Ngami, a point equidistant from the eastern and western coasts. He congratulated himself upon the peculiar advantage of his seeming to be a traveller toward the country of the whites—homeward; for the savages can comprehend the utility of such a journey, and unless this is clear to them, they become suspicious and anxious to drive the stranger from their borders. In view of so important an advantage, this route cannot be too highly recommended to such as are in a situation similar to that of Dr. Livingstone, or do not dread the expense of a preparatory journey from the Cape to Lake Ngami; that is, to such as are willing to spend some time in that unhealthy region, in order to become acquainted with the country and its inhabitants, and then return once more to the cape to prosecute the final preparation for a more extensive expedition. North of Livingstone's route one can not expect to meet with a beaten path or with other travellers, hence the preparations must be more extensive, many requisites for the journey must be carried by the party, circumstances which will considerably increase the expenses. To other travellers this route will prove less desirable, from the fact that on the eastern coast there is a point which presents all the facilities requisite for the fitting out of an expedition; at this point too, the means of communication with Europe and the interior are adequate, nor will the traveller here meet with that great obstacle, the hostilities between the Mohammedan and natives. Nor here will he expose himself to a loss of life and health. Such are the advantages that the island of Zanzibar presents.

From this general survey of the difficulties of travel in Central Africa, Dr. Roscher proceeds to consider in an interesting manner, the characteristics of Zanzibar, but our limits do not permit us to follow this portion of his remarks.

North and Central Australia. Gregory's Expedition. Leichhardt's Fate.—In the last number of this Journal some account was given of recent explorations in South Australia, especially in the neighborhood of Lake Torrens.

We have since received from Rev. W. B. Clarke of Australia, a communication addressed by him to the Sydney Morning Herald of Sept. 10, 1858, in respect to the probable fate of Leichhardt, which gives us an occasion to refer to the explorations in the northern and central parts of the continent.

It is well known that the question has been much discussed in respect to Australia as well as in respect to Africa, whether or not in the interior of the continent, a great sea exists. Eyre, whose explorations were made in 1840 et seq., adhered to the belief that no such sea existed. On the other hand, Sturt, journeying north from Adelaide, 1844–6, although failing to discover an actual sea believed in its existence. The following remarks are given in his own words.

"The principal features of the interior are the sandy ridges or dunes, by which it is traversed from south to north, and the Great Stony Desert. That the whole region traversed was once submerged, there cannot, I think, be a doubt. Its salsolaceous productions, its sea-level, its want of trees of any size or growth, excepting on the banks of the creeks, sufficiently attest this; but whether the sandy ridges were thrown up simultaneously, or were successively formed by a joint effect of winds and a gradually retiring sea, or of winds alone, it is impossible to say. When I first crossed the Stony Desert, it appeared to me to have been the bed of a former current; and I felt satisfied that the conclusion was just when I crossed it at another point more than a degree from the first, and noticed the strong proof it exhibited of waters having at one time or other swept over it with unresistible fury. Whether the Stony Desert continues to any distance I cannot say, but my opinion is that it does, and that, as the lowest part of the interior, *it receives all the waters falling inwards from the coast.* Whether those waters are gradually lost by evaporation, or that they are carried to some still undiscovered sea, remains to be proved; but as it is difficult for others to elucidate these things, I have thought myself called upon to throw every light I can on the probable character of the interior. All I can say is, that after having traversed a desert for 400 miles and failed to reach its northern limit, and after having found that it continued unaltered for four degrees of longitude,

I cannot hope that it speedily closes in, either to the east or west."

In 1848, Leichhardt, whose previous journeys had made important additions to our knowledge of the northeastern parts of Australia, set out with a party of eight men to cross the continent from east to west, expecting to be gone two years and to determine the great question in respect to the interior. No news has been received from him since a short time after his departure. Hopes were entertained that Gregory's expedition would bring some news of his course and fate, but these expectations are disappointed.

An outline of Gregory's tour is thus given by Sir R. I. Murchison, on awarding to him the Founder's gold medal of the Royal Geographical Society of London.

"Having ascended the Victoria as far as was practicable, Mr. Gregory established a camp on the right bank of this stream, and at about 80 miles from its mouth. With his brother, Mr. H. Gregory, Mr. Wilson the geologist, and Dr. Ferdinand Mueller the botanist, he then explored the Victoria to Jasper Creek, determining the geological nature of the country, and ascertaining that the river made a great southward bend. Again taking with him his brother, and Dr. Ferdinand Mueller, together with the artist, Mr. Baines, he marched southwards to ascertain if the saline desert, which Sturt had discovered in proceeding inland from the southern regions of Australia, and which he had himself found to prevail in Western Australia, was also to be met with in a journey southwards from the north coast.

"For this purpose he ascended the Victoria to its source, and found the hilly or dividing range to have an altitude of 1660 feet above the sea. Traversing this watershed, he descended by a south-flowing stream, which he named Sturt Creek, and which, bending to the S.S.W., terminates in a desiccated salt lake near Mount Wilson, in S. lat. 20° 2′ and E. long. 127° 5′. Whilst the southeastern and southern slopes of the dividing range were thus proved to be everywhere dry and sterile sands, the whole of the territory to the north of the same presented the most striking contrast, being generally very fertile in grasses, particularly the extensive grounds named Hutt Plains and Roe Downs.

"In this first effort, therefore, made specially by the advice of our medallist Sturt, the grand geographical and statistical feature which was suspected to exist was brought to the test; and we may now fairly infer, that all the central portion of this continent, as well as the long southern coast-line examined by Eyre, and a considerable maritime frontier of Western Australia, constitute an uninhabitable desert, probably the dried-up bottom of a sea, and that hence all future intercourse between our Australian colonies must take place either along the fertile coast ranges, or by sea.

"Returning to his camp, which he had left under the charge of Mr. Wilson, the geologist of the expedition, who had in the mean time examined the adjacent country, of which he sent home sketch maps to this Society, Mr. Gregory sent away Mr. Baines the artist, with Mr. Wilson, and the larger number of his party, in the schooner; and after giving directions that the vessel should meet him at the head of the Gulf of Carpentaria, he set out on his chief mission, accompanied by his brother, Mr. Elsey the surgeon, Dr. Mueller the botanist, and three men.

"Quitting the basin of the Victoria, and passing over a broad table-land of sandstone, he entered a valley watered by a tributary of Leichhardt's river the Roper, which he named Elsey Creek, in S. lat. 15° 15′ and E. long. 133° 10′. He next took a south-southeasterly direction to the west of Leichhardt's route, or about 70 miles distant from the western shore of the Gulf of Carpentaria, and traversed the various rivers discovered by his adventurous precursor (but nearer to their sources) until he reached the Albert, which empties itself into the head of the Gulf. Not meeting there with the party sent by sea, under the orders of Mr. Baines, he left the 'Plains of Promise' of Stokes, and crossed the river Flinders at about 80 miles distance from the Albert, and, journeying to the northeast, fixed a position on the Gilbert river at S. lat. 18° 0′ and E. long. 140° 40′. Ascending that stream, Mr. Gregory left behind the drainage into the Gulf of Carpentaria, and traversed the high basaltic plateau which separates the waters flowing into that gulf from those which descend into the great eastern ocean. To the dividing high lands he assigned the name of 'Newcastle Range,' in honor of the Secretary of State for the Colonies, who had sanctioned and organized the expedition. Reaching the Burdekin, he followed that stream southeastwards to its junction with the Cape river of Leichhardt.

"The next march showed the connection of the Suttor of Leichhardt with the Belyando of Mitchell; then striking southwest from the latter stream, Mr. Gregory skirted the Peak range, the extreme point to which squatters have extended their dwellings, *i. e.* in S. lat. 23° 41′ and E. long. 147° 50′, or about 560 miles from the head of the Gulf of Carpentaria.

"Whilst a great breadth of entirely sterile tracts, with one insulated rich spot only on the river Roper, prevail between the basin of the Victoria on the north coast and the Gulf of Carpentaria, with occasional poisonous plants, Mr. Gregory found nearly all the vast region between the eastern side of the gulf and the northernmost station of our settlers to be more or less fertile. So that in the last weeks of the expedition the horses fattened, and after traversing the rivers Mackenzie, Comet, Dawson, and Burnett, the party reached the Brisbane and Moreton Bay in excellent health."

The publication of Mr. Gregory's report, without any decisive indications of Leichhardt's route, led Mr. Clarke to prepare the paper already referred to in the hope that the discussion might initiate another expedition into the district yet unexplored, between the 145th and 147th meridians, and which, as he conceives, may bear traces of Leichhardt's route, although the explorer probably perished far to the northward and westward of the Victoria. We have not room to make extended extracts from this paper. Its whole tendency is to show that the intention of Leichhardt was to skirt and not cross the desert. There is an extended criticism to show the probability that an L found by Gregory on the Victoria, and two marks XVA in an L-shaped border previously found near the Warrego and the Nive were not the marks of Leichhardt. Various independent witnesses are also cited to confirm the opinion of Mr. Clarke that Leichhardt did not attempt to intersect the desert. The article concludes as follows.

"If then he did reach the Victoria in 1848, and was not cut off, and we have now no ground to conclude he was, he would, in case of finding the country impracticable to the west, have gone round by the head of the Victoria, towards the north, and it is somewhere between the head of the Victoria and the head of the Clark, that, I think, his tracks are to be looked for; not, probably, on any line of route explored by Mitchell, but to the westward, or, crossing Mitchell's track, on a line to Peak Range and the Burdekin. Or driven in by drought, he may have taken a course on the 148th meridian, without going across the Maranoa, where Hely could not trace him, on a new track of his own.

"It is in the hope that a new expedition may be organized, with a view to the exploration of the country west of Mitchell's Belyando, as well as to ascertain whether any traces of my lamented friend can be found, that I have made this communication; believing that, as I have had it in my power to put the matter in a tangible form, and to quote from manuscript notes in my possession, what has not before been committed to the press, I am only rendering a service to the cause of science, civilization, and humanity."

The following paragraph may be read with interest in this connection. It is a letter of Dr. J. Palacky of Prague to Prof. C. Ritter of Berlin, printed in Poggendorf's Annalen für Chemie, vol. 100, 1857.

"The statement of Sturt that Lake Torrens must lie below the level of the sea, led me to think that Central Australia must lie very deep. I found unfortunately no other data than in Sturt, Bd. ii, p. 299, where in connection with Kennedy's route to the Victoria river—it is mentioned that in lat. 25° 55′ 37″ and long.

142° 24′, the water in the camp when the air was at a temperature of 64° Fahr., had boiled at 214° F. Prof. Coritska, who directs the height measurements of our Geological Reichsanstalt, undertook at my request the computation of these data and found that if they are correct, this point must lie 306 meters below the sea level."

ART. XXIX.—*Analysis of the White Sulphur Water of the Artesian Well of Lafayette, Indiana;* by CHARLES M. WETHERILL, Ph.D., M.D.

History of the Well.—The artesian well of Lafayette is situated in the northeast angle of the court-house square of the city. The boring was commenced in the spring of 1857 by order of the Commissioners of Tippecanoe county to test the feasibility of artesian wells for prairie farms, and with prospects of success from the results of the wells in Illinois to the southwest. On February 18th, 1858, after ten months labor, a vein of overflowing water was struck in the grey limestone, at a depth of 216 feet 6 inches. The depth of the well was subsequently increased to 230 feet without any change in the character of the water, which is very similar to that of the celebrated Blue Licks, of Kentucky. The strength of flow of the water began to diminish, and while injudiciously boring for more water instead of trying to stop a known leak, the water suddenly ceased to overflow, and fell in the well about 20 feet. As there was no prospect of the recovery of the water by the contractor, the County Commissioners placed the well in the hands of Captain Rogers and myself, for the purpose of obtaining some experimental knowledge with respect to the source of the difficulty. Before giving the results of our experiments, I will submit a table which shows the order and nature of the strata encountered in boring the well:

Table of Strata encountered in the Lafayette Well.

	Feet.	Inches.	Remarks.
Clay	3		
Clay and gravel	9	6	Water.
Gravel and pebbles	1	6	
Fine gravel and sand	13		
Quicksand	1		
Gravel, clay and pebbles	2	6	
Dark gray clay	72		Marlite.
Sand and gravel	4		Water.
Clay and pebbles	1	3	
Sand and gravel	7	3	
Clay		6	

	Feet.	Inches.	Remarks.
Sand and gravel	3		
Clay and pebbles	6	6	
Gravel and pebbles	5		
Boulders	40		
Blue shale	2		To shale 170 feet.
Gray "	18		Iron pyrites in all the rock
Blue "	1	6	formation.
Gray "	7		Thickness of shale 28 ft. 6 in.
Limestone—coralline	11	6	To coralline, 198 ft. 6 in.
Gray limestone with spar	20		Overflowing water.
Depth of well from original surface	230		

The first water in the table is the well water of the locality. The water under the marlite is fresh; it was expected to overflow but only rose to within 36 to 38 feet of the surface. The third and overflowing water is the mineral water.

A cast iron pipe of eight inches diameter cases the well to the first rock, which is a friable blue shale. The hole in the rock was not tubed, it was of $7\frac{1}{2}$ inches in diameter to near the commencement of the coralline; for a few feet farther it was of five inches, and for the remaining distance of twenty-one feet its diameter was four inches. We found that the bottom of the iron pipe had been broken during its passage through the forty foot bed of boulders, and as this fractured end rested upon a friable bed of blue shale with porous strata immediately above, a leak of the mineral water had always existed. From an experiment made during the overflow of the water we were able to assign a delivery of eighteen thousand gallons during twenty-four hours as the lowest capacity of this leak. We found that the cessation of overflow was caused by the leak having been increased by the removal of a piece of the shale rock. Restoring the overflow by stopping this leak was the natural inference from the experiments upon the well; but there were difficulties in the way owing to the bad shape of the hole in the rock, by the use of improper boring tools. The holes were all of triangular shape, and the four inch hole was especially bad, having reëntering sides and semicircles at the three angles. We might have arrested the leak by tamping and puddling at the end of a long tube below it, but were fearful of presenting difficulties thereby in the case of the removal of such a tube, for as the leak at the top of the shale rock had always existed, we had no means of knowing whether some of the mineral water was not derived from the shale, and would be shut out by such a tube.

The leak was at length arrested by the following device of my own, which I submit in the event of a future need for it, as it proved simple and successful in the Lafayette well, and I am not aware that it has ever been employed in a similar case. The

commencement of the four inch hole was made truly cylindrical and of six inches in diameter for a distance of nineteen inches. A car spring of vulcanized india rubber was turned in a lathe to the shape of the frustum of a cone six inches in length, and of diameters six and a quarter and five and seven-eighths inches. Upon a wrought iron gas pipe of two and a half inches bore and of sufficient length to reach the coralline rock, was chised a screw thread upon which ran two heavy nutts. The hole in the rubber plug was turned sufficiently large, that the plug might be screwed tightly upon the gas pipe, where it was secured for pulling or pushing by the nutts above and below it. When the pipe was placed *in situ* in the well, the plug was arrested in its rocky socket just above the commencement of the coralline rock. The water at once fell outside of the gas pipe, and rose on the inside to near the surface of the ground. After two days it commenced to overflow and increased constantly and steadily. The increase at first was very rapid, the delivery doubling itself in five hours; a repeated and careful measurement of the quantity of water showed that the *rate of increase* fluctuated, and became gradually less. This compelled the inference that the leak had lowered the head of water in the reservoir supplying the artesian well, which head was gradually restored by the rains, springs, &c., upon shutting off the leak. This increase in the flow will go on until the overflow equals the feed of the reservoir. When the delivery was last measured on December 8th, it equalled one wine gallon in 14·77 seconds, or 5850 gallons in twenty-four hours.

Geology of the well.—The order and character of the rocks in proceeding from a point west to one east of Lafayette, is from information furnished by D. Brown, State Geologist, as follows:

1. Seams of Coal.
2. Mountain Limestone about 200 feet in thickness.
3. Clay Sandstone (Devonian) about 500 feet.
4. Delphi Slate varying from 25 to 100 feet, and thinning to the northwest.
5. Grey Limestone (Upper Silurian).
6. Blue Limestone (Lower Silurian).

The dip of these rocks is about 25° to the south of west, and at an angle of 50 feet fall to the mile. This westward dip is maintained until the Mississippi is crossed, when the dip is eastward. The sequence of the rocks, in penetrating the earth vertically at Lafayette, should be Clay Sandstone, Delphi Slate, and Grey Limestone; but the valley of the Wabash at the artesian well is about one hundred feet below the general elevation of the country, and the force which has scooped out this valley has removed the Clay Sandstone, not a trace of which was discovered in boring the well. The Delphi Slate was the first rock reached

after traversing the drift, and the bottom of the well is situated in the upper measures of the grey limestone. A crude idea of the strike of these rocks may be gained by tracing upon a map of Indiana the strike of the Delphi slate, by a curved line joining Louisville, Ky., Lexington, Ind., Elizabethtown, Indianapolis, Delphi and Crown Point, Ind., which shows that the well is situated upon the edge of a great geological basin. As far as our present geological knowledge goes, (and which is limited owing to a backwardness on the part of the State Legislature in making the appropriations necessary to a survey,) the reservoir of the artesian well must lie in the direction of and beyond Delphi, for the water comes from below the slate cropping out at this locality. A glance at the map will show by the river courses, that Kokoino is the highest ground in the neighborhood; hence we shall probably not be far from the truth in assuming the reservoirs to be situated somewhere in the triangle formed by joining Delphi, Logansport and Kokoino. During the experiments upon the well by Captain Rogers and myself, one of the greatest freshets in the Wabash, within the memory of the inhabitants of the valley took place. The leak had not yet been stopped, and we found that the water in the well rose and fell simultaneously with the freshet. It is reasonable to suppose that this effect of the freshet was not upon the mineral waters but upon the water under the marlite (see table of strata); for the smaller freshets in the river since the leak has been stopped have not in the least affected the flow of the well. This can only be accounted for by supposing that the 72 foot bed of dark grey clay crops out under the Wabash at a level lower than that of the ground at the well. It follows from this that we cannot expect an overflow of this marlite-water in the neighborhood of Lafayette. In fact since the completion of the mineral well, three other wells have been dug in the city for the expressed purpose of reaching the marlite-waters. It was obtained in every instance, but did not overflow.

CHEMICAL ANALYSIS OF THE WATER.

Physical Characters.—This water is of extreme limpidity when taken freshly from the well. The deposit upon the pebbles over which it flows is *white*, entitling it to the name of a *white sulphur* water. Standing in imperfectly closed vessels, a similar bluish white deposit takes place, which under certain conditions contains black flakes of sulphuret of iron. The smell of the water is strongly of sulphuretted hydrogen. The taste is similar to that of the celebrated Blue Lick water, though less strong. It is pleasantly brackish, resembling in taste, the liquor from oysters freshly opened. The density, from a mean of six observations is 1·00523.

The temperature, noted at intervals since the water was first obtained, (Feb. 18th to December 8th,) remained constantly between 55° and 56° F., my thermometer not being sufficiently delicate to give more definite results.

Although the mean temperature of Lafayette is unknown,* I have no doubt from other considerations, that the artesian water is "thermal;" for, first, the calculated temperature of the water upon the Grenelle basis renders this, in absence of contradictory facts, most probable. It will be remembered that thermometers placed in the wells of the Paris observatory stand invariably at 53° F., at a depth of ninety feet, and that from the increase of temperature at increased depths observed in boring the Grenelle well, the fact was established that the temperature rises one degree F., for every $61\frac{3}{10}$ feet, after the first ninety feet. After taking ninety feet from the depth of the Lafayette well there remain 140, and if the same ratio of increase of temperature exists as at Grenelle, the water should have a temperature of $2\frac{1}{4}$° above 53°=55$\frac{1}{4}$°, which agrees closely with the temperature actually found for the Lafayette water. Secondly, we infer that the artesian water is "thermal" from the fact that its temperature is above that of the neighboring springs and wells, as may be seen from the following table, which contains wells and springs situated at different points of the compass from the artesian well, and within a circle of two squares radius.

Temperature of the Wells and Springs of Lafayette, taken April 30, 1858.

	LOCALITY.	Depth Feet.	Direction from Artesian well.	TEMPERATURE.	
				Air.	Water.
Well,	Mr. C. Taylor's dwelling,	25	South	78° F.	51° F.
Well,	Courthouse yard,	16	Southeast	"	51°
Well,	Wilstach's Drug Store,	16	North	"	48°
Spring,	Two squares from Artes. Well,		North	81°	50°
Well,	Mr. Benbridges' dwelling,	20	Southeast	80°	49°
Spring,	Messrs. Taylor & Co., lumber yard,		Northwest	"	50°
Well,	Cellar of Mr. J. Mix's store,	16	West	56°	49°
Well,	Lahr's Hotel,	16	East	80°	50°
Artesian w'll,				80°	55-56°

Chemical Characters. Qualitative.—The water is faintly acid from sulphuretted hydrogen and carbonic acid, but becomes neutral after having been boiled, owing to the expulsion of these gases. It follows from this fact that *all* of the sulphur is in the state of sulphuretted hydrogen dissolved in the water, and from the neutrality after boiling, that alkaline carbonates are absent. A particular experiment for the search of a trace of alkaline carbonates gave the same results.

Carbonic acid is contained dissolved in the water, and holding in solution the earthy carbonates. Nitrogen is the only remain-

* The mean temperature is probably 51° to 52° F.—Eds.

ing gas in solution. On boiling the water the carbonates of lime, magnesia and iron, held in solution by the carbonic acid, are thrown down, and by a slight concentration, sulphate of lime is also precipitated. In the boiled water chlorids of sodium, calcium and magnesium, were detected. Crenic and apocrenic acids were in vain sought. A trace of organic matter was found dissolved in the water. The only alkali present is soda in the state of chlorid of sodium. No trace of potassa was discovered after a careful search. Mr. H. C. Lawrence kindly undertook the concentration of eleven and a half wine gallons of the artesian water, which were boiled down to half a gallon; a concentration in the ratio of 23 to 1. In the solid residue and filtrate, I detected phosphate of lime, hydrofluoric acid, alumina and a very faint trace of oxyd of manganese. A small trace of iodine was discovered in the mother liquid, both by the starch test and by chlorid of palladium. With the starch test the characteristic blue tinge could not be developed by chlorine water, the excess of chlorine decolorizing the extremely minute quantity of iodid of starch; but it was readily brought out by nitric acid. The result of the bromine test by Fresenius's method was doubtful.

Quantitative analysis.—The quantitative analysis gave the following results:

	Per mill.
Mean of two exp., sulph. acid determination,	0·5621
" " chlorine,	3·7807
Sodium by calculation, (loss,)	2·1782
" " experiment,	2·1683
Peroxyd of iron, mean of two exp.,	0·0085
Silica,	0·0080
Lime—total,	0·5386
" after boiling the water, in the precipitate,	0·1149
" " " " " in the solution,	0·4255
Magnesia—total,	0·2005
" after boiling the water, in the precipitation,	0·0039
" " " " " in solution,	0·2088

Sulphur, carbonic acid, and nitrogen as stated below. These data calculated according to the ordinary rules give the following result:

Composition of the White Sulphur water of the Lafayette Artesian well. Water of March 25th, 1858.—Temperature 55°–56° F. Density, 1·00523.

GASEOUS CONTENTS.

	In 1000 grams. Grams.	In 1000 grams. Cub. centim.	In a wine pint. Cubic inches.
Sulphuretted hydrogen,	0·0093	6·3594	0·1841
Ditto water of April 8,	0·0145	9·9154	0·2870
Carbonic acid,	0·0997	52·683	1·5253
Nitrogen,		21·280	0·6160

SOLID INGREDIENTS.

	In 1000 parts by weight.	Grains in a wine pint.
Residue by evaporation, Pure water,	992·75	7274·446
Solid ingredients,	7·25	53·124
	1000·00	7327·570*

INGREDIENTS BY ANALYSIS.

Carbonate of lime,	0·2052	1·503
Carbonate of magnesia,	0·0069	0·050
Peroxyd of iron with alumina, Phosphate of lime, fluorid of calcium, And a faint trace of manganese,	0·0085†	0·062
Silica,	0·0080	0·058
Sulphate of lime,	0·9555	7·002
Chlorid of calcium,	0·0635	0·465
Chlorid of magnesium,	0·5059	3·707
Chlorid of sodium,	5·5402	40·596
Trace of iodine and organic matter, Bromine doubtful,		
	7·2937	53·443

I have recalculated the analyses of the principal sulphur waters of the United States to the same measure, a wine pint, and tabulated them as follows, for the sake of a ready comparison: (see table on following page).

By reference to the table, the great analogy is at once apparent which exists between the Lafayette water and that of the Kentucky Blue Lick. They contain, with a few trifling exceptions, the same ingredients. The exceptions are the sulphate of potassa and chlorid of potassium, contained in the Blue Lick alone, and the chlorid of calcium, contained above in the Lafayette water. The latter water contains less sulphuretted hydrogen, and carbonic acid, and less solid matter. It is curious that the common salt bears almost exactly the same ratio to the rest of the salts in both waters.

Total Salts.				Common Salt.		
Blue Lick.	Lafayette.			Blue Lick.	Lafayette.	
79	: 53	: :		64	: x=42	

The common salt (x,) in the Lafayette waters, is by experiment nearly 41.

In round numbers one and a half pints of the Lafayette water contain as much common salt as one pint of the Blue Lick water. The magnesia salts bear a greater proportion to the rest of the salts in the Lafayette water than in the Blue Lick.

* This is the weight of a wine pint of the artesian water; the weight of the same measure of pure water being 7291·11 grains.

† Equivalent to carbonate of the protoxyd of iron 0·0061 per mille.

Tabular view of the principal Sulphur Waters of the United States. Expressed in grains in the wine pint.

Name of Springs.		Temperat. F.	Density.	Carbonates.			Sulphates.				Chlorids.				Iodid.	Bromid	Name of analyst.
				Lime.	Magn.	Bic'b. Mag.	Lime.	Mag.	Potas.	Soda.	Calc.	Mag.	Pot'm	Sodium	Magnesium.		
Sharon, N. Y.	Sulphur Spring,						6·98	2·65				0·15		0·14			Chilton.
	Magnesia Spring,					3·81	9·50	2·83				0·36*					L. Reed.
Avon, N. Y.	New Spring,	50°	1·00356	3·37			0·44	1·01		4·84				0·71			Hadley.
	Middle Spring,	51°		1·00			10·50	1·25		2·00				2·30			Hadley.
	Lower Spring,	45°–47°	1·0018	3·58	....		7·17	6·21		1·71	1·05						Chilton.
	Sylvan Spring,			3·35	2·0		10·05	1·62				7·8		12·18	trace.		Chilton.
Virginia,	Greenbrier White Sulphur,			1·15			7·744	5·588			0·204			0·180			Rogers.
Kentucky,	Blue Lick,	62°	1·007	2·957	0·017		4·25		1·117			4·049	0·174	64·107	0·005	0·030	Peters.
Indiana,	Artesian Well, Lafayette,	55°–56°	1·00523	1·503	0·050		7·002				0·465	3·707		40·596	trace.	†	Wetherill.

Name of Springs.		Metallic Sulphurets.	Oxyd iron, etc., etc.	Silica.	Organic matter.	Loss.	Total grains in a wine pint.	Cubic inches of gas in a wine pint.				Name of analyst.
								Sulphuretted Hydrogen.	Carbonic acid.	Nitrogen.	Oxygen.	
Sharon, N. Y.	Sulphur Spring,	0·14†					10·06	1·00				Chilton.
	Magnesia Spring,	0·06‡					16·57	0·41				L. Reed.
Avon, N. Y.	New Spring,						10·37	3·91				Hadley.
	Middle Spring,						17·05	12·00	5·60			Hadley.
	Lower Spring,						19·72	1·32	0·50			Chilton.
	Sylvan Spring,				0·03		37·03	2·58	0·62	0·67		Chilton.
Virginia,	Greenbrier White Sulphur,		trace.		trace.	0·41	15·276	0·175 to 0·343§	0·25	0·444	0·181	Rogers.
Kentucky,	Blue Lick,		0·045	0·137	trace.	2·216	79·104	0·834	5·837			Peters.
Indiana,	Artesian Well, Lafayette,		0·062	0·058	trace.		53·443	0·184 to 0·287	1·525	0·616		Wetherill.

* Chlorids of Magnesium and Sodium.

‡ Sulphurets Calcium and Magnesium.

† Sulphurets Calcium and Sodium.

§ 0·032 by Dr. Hayes' analysis.

The sulphuretted hydrogen of the Lafayette water is equal in quantity to that ingredient in the Greenbrier White Sulphur water of Virginia, and varies as in that water. I established this fact by many and careful sulphur determinations of the Lafayette water by the chlorid of arsenic test upon water taken at the spring. I have also made frequent careful density determinations of the water during a period of six months, and have found the specific gravity invariably to the third decimal point, proving an invariable mineral composition for the water during that time.

The Lafayette water has been used with great success in the diseases for which sulphur waters are applicable.

I have noticed in the neighborhood of this city several chalybeate springs. A very fine one is situated upon the shore of Barnett's creek, which flows through the celebrated battle-ground of Tippecanoe at a distance of seven miles from Lafayette. The temperature of the water was 53° when that of the air was 84°. It strikes a dark color with extract of galls, has a strong chalybeate taste, and coats the stones over which it flows with an ochreous deposit.

ART. XXX.—*On the Measurement of the Striæ of Diatoms;* by W. S. SULLIVANT and T. G. WORMLEY.

No characters are so constant for distinguishing the species of diatoms as those drawn from the striæ on their frustules. The accurate measurement of these striæ has not received the attention, particularly among European microscopists on the continent, that might have been expected from the general interest taken in the study of these beautiful organisms.

Attention appears to have been first directed to this subject in Silliman's Journal for 1849–50, by the late distinguished Professor J. W. Bailey of West Point, and Mr. De La Rue of London, in their papers on the marking of Pleurosigma Spencerii, the measurements of which, made by the latter gentleman, remaining to this day a reliable standard for comparison. In 1853 the first volume of Smith's admirable synopsis of British Diatomaceæ appeared, in which the striation of numerous species, is, for the most part, correctly determined. Next in order of time is the paper of Messrs. Harrison and Sollitt, read (1854) before the British Association for the Advancement of Science, giving measurements (widely different from those in Smith's Synopsis) of several well known species.

The above, including the second volume of Smith's Synopsis, embraces about all the information on record, relating to the subject of these papers, known to the writers.

The measurements of the larger portion of the following species were made on authenticated English specimens. The figures affixed to each species express the number of transverse striæ in the $\frac{1}{1000}$th of an inch (·001″), as determined by us; next following and in parentheses are the measurements, if any, of other observers. In not a few cases it will be seen that our measurements accord very nearly with those in the Synopsis, not differing more than might be expected in observations of this kind: besides it is well known that the striation of each species varies within certain limits peculiar to the species. In other cases, however, there are discrepancies between our measurements and those in the Synopsis, too great to be accounted for in this manner.

Nitzschia sigmoidea, 72 to 75; (85, Smith); (105, Harrison and Sollitt).—N. obtusa, 54; (56, S.).—N. plana, 50; (56, S.).—N. linearis, 73.—Stauroneis linearis, 65.—Cocconeis Thwaitesii, 54; (72, S.).—Amphora membranacea, hoop 53, valve 36; (80, S.).—Cymbella cuspidata, 35; (30, S.).—C. Scotica, 45; (42, S.).—Navicula lanceolata, 40; (44, S.).—N. firma, 42; (42, S.).—N. ambigua, 38; (36, S.).—N. sphærophora, 40; (42, S.).—N. lævissima, 51; (48, S.).—N. rhomboides, 70; (85, S.).—Pinnularia Johnsoni, 48; (56, S.).—Fragillaria virescens, 40; (44, S.).—F. Capucina, 40; (40, S.).—Colletonema vulgare, 72; (72, S.).—C. eximium, 56; (56, S.).—Achnanthidium coarctatum, 34; (40, S.).—A. lanceolatum, 33; (40, S.).—Amphipleura inflexa, 52; (52, S.).—A. pellucida: we have not been able even to "glimpse" the striæ on this diatom. Messrs. Harrison and Sollitt in their paper above cited, estimate the striæ at 125 to 130 in ·001″.—Himantidium pectinale, valve 27, hoop 48; (v. 27, h. 48, S.).—Pleurosigma macrum, 70; (85, S.).—P. angulatum, 45 to 50; (52, S.); (75, H. & S.).—P. Spencerii, 48 to 50 tr., 55 long.; (52 tr., 55 long., S.); (120 to 200, Bailey).—P. attenuatum, 36 tr., 32 long.; (40 tr., 30 long., S.).—P. fasciola, 56; (64, S.); (90, H. & S.).—P. littorale, 40 tr., 22 long.; (50 tr., 24 long., S.).—P. acuminatum, 45 tr., 40 long.; (52 tr., 40 long., S.).—P. strigosum, 42; (44, S.).—P. Hippocampus, 38 tr., 32 long.; (40 tr., 32 long., S.).—P. lacustre, 42; (48, S.).—P. quadratum, 45; (45, S.); (70, H. & S.).—P. speciosum, 42; (44, S.).—P. prolongatum, 55; (65, S.).—P. strigile, 30 tr., 35 long.; (36 tr., 40 long., S.).—P. elongatum, 48 to 50; (48, S.); (60, H. & S.).—P. distortum, 60; (75, S.).—P. formosum, 36 tr., one set of oblique striæ 24, the other 30; (36, S.).—P. decorum, 45 tr., 36 oblique; (36, S.).—P. intermedium, 56; (55, S.).—P. Balticum, 36; (38, S.).

—P. Wormleyi, 52.*—P. obtusatum, 56.†—P. Sciotoensis, 40.‡—Synedra radians, var.? a common fresh water species in this vicinity, has on the hoop about 75 striæ in the ·001″, very difficult, when balsam mounted, to resolve, owing probably to their shallowness.—Grammatophora subtilissima, 70 to 75 on the Greenport variety, 75 to 82 on the Providence variety.—Hyalodiscus Californicus: a large valve ·005″ in diameter (the central disc or umbilicus measuring ·0016″) gave at the margin of the umbilicus about 65 straight radial lines in ·001″; midway between that point and the circumference of the valve near 70 in ·001″; and at the circumference about 75 in ·001″. More or less of these lines from the margin of the umbilicus, before reaching the circumference, bifurcate: this accounts for their being coarser near the umbilicus than at the circumference, as is shown by the above measurements, and by the greater ease with which they are resolved near the former point. The umbilicus itself is rayed, and in certain portions of the light exhibits curved lines.

To those interested in these matters it may not be unacceptable to state the means and method by which the above measurements were obtained. The objectives used were two $\frac{1}{12}$ths, one by Spencer, the other by Ross, and a $\frac{1}{25}$th by Powell and Lealand, upon one of Smith and Beck's first-class stands, with their second eye-piece and Jackson micrometer. An important adjunct to the above was Tolles' amplifier which consists essentially of an achromatic meniscus lens placed between the objective and the eye-piece, affording an amplification much superior to that from ordinary eye-piecing. The value of the Jackson micrometer (a scale of ·005″) with each of the objectives was carefully established by the averages of numerous trials on two of Smith and Beck's stage scales of ·001″. Thus the smallest divisions of the Jackson micrometer were made by moderate

* Pleurosigma Wormleyi, Sulliv.—Lanceolatum, conspicue sigmoideum, in apices acutos subito attenuatum; long. ·003″; striis trans. et long. 52 in ·001″.

Fresh water; Columbus, Ohio, *Prof. T. G. Wormley;* Genessee river, N. Y., *Mr. C. A. Spencer.*

Resembles P. Spencerii, but is a smaller species, more evidently sigmoid and with rather abruptly attenuated ends: its striæ are more difficult to resolve, being shallower: texture of the valves thinner.

† Pleurosigma obtusatum, sp. nov.—Oblongo-lanceolatum, leviter sigmoideum, apicibus obtusis; long. ·0025″; striis trans. et long. 56 in ·001″.

Fresh water; Columbus, Ohio: Gambier, O., *Prof. H. L. Smith;* Genessee river, *Mr. C. A. Spencer.*

A very small species remarkable for the obtuse ends. It may be a Colletonema, but we have not observed it in gelatinous envelopes.

‡ Pleurosigma Sciotoensis, sp. nov.—Lineare, modice sigmoideum, in apices obtusiusculos sensim attenuatum; long. ·005″; striis trans. et long. 40 in ·001″.

Fresh water; Columbus, Ohio: Genessee river, N. Y., *Mr. C. A. Spencer.*

Not unlike P. Spencerii, for which it has passed as a variety, but is a larger species, its valve having more parallel sides and less acute ends. Its striation at once distinguishes it. Dry valve pale straw-color.

draws of the draw-tube to measure with different objectives from ·000025″ to ·00000167″ under an amplification of 1700 to 3600 times; precautions being taken against any disturbance of said value, that might arise from the thin glass covering the object. A Powell and Lealand cobweb micrometer was occasionally used, but mostly for corroboration. The Jackson micrometer, upon the whole, was found to be more manageable and satisfactory, the shortness of its lines permitting their coincidence with the striæ to be more readily observed. The light employed was sunlight, admitted through a small aperture into a darkened room: with no other light could the striæ of the highest marked frustules be distinctly resolved under an amplification sufficient for counting. In order to diminish as much as possible the chances of error, the counting always embraced as many contiguous striæ as covered a space equal, at least, to that between the centres of the *five-lines* of the micrometer, and when practicable a larger number was counted.

In conclusion, we may remark that our experiments are confirmatory of the generally received opinion that striæ closer than about 85 in ·001″ have not yet been resolved. Whether this limit is interposed by the physical properties of light, or whether it arises from defects still existing in our apparently faultless instruments, remains to be determined.

Columbus, Ohio, January 18th, 1859.

Art. XXXI.—*Correspondence of Prof. Jerome Nicklès, dated Paris, January 3rd*, 1859.

On the nature of simple bodies.—The Comptes Rendus for December contains a long memoir by Despretz on his researches to ascertain whether certain of the so-called elements are decomposable. His laborious and careful investigations have led to no decomposition, and he announces the conclusion that the substances called elementary are really elementary or incapable of decomposition. The author should have added, that they were not decomposable by the methods he used, for it is not probable that there is nothing more to be done in this branch of research. His process consists in submitting the element—cadmium for example—to the physical and chemical reagents ordinarily employed in analysis. He transforms it into an oxyd, then into salts of all kinds, decomposes these salts by chemical and galvanic methods, precipitates the metal at one time at the positive pole, at another at the negative, examines the crystalline form, turns it again into salts, which he decomposes, vaporizes the metal by means of the pile; and thus causes an element to pass through a great number of different states, and still arrives at the same element. While rendering justice to the zeal and patience of Mr. Despretz, we have to regret that these good qualities have been here wasted,

for the researches would be a hindrance to the progress of science if taken seriously.

Dumas took upon himself the refutation of Mr. Despretz, and brought to the subject his well known ability. He prefaced his remarks by presenting the following table which exhibits an interesting relation between the equivalents of certain simple and compound bodies.

Fl 19	Cl 35·5	Br 80	I 127	} Difference 5.				
N 14	Ph 31	As 75	Sb 122					
Mg 12·25	Ca 20	Sr 43·75	Ba 68·5	Pb 103·5	} Difference 4.			
O 8	S 16	Se 39·75	Te 64·5	Os 99·5				
Ammonium 18	Methylamine 32	Ethylamine 46	Propylamine 60, etc.	} Diff. 8.				
Methylium 15	Ethylium 29	Propylium 43	Butylium 57, etc.					

As this relation suggests a doubt as to the elements being simple, Dumas took occasion to express his opinion on this important question.

Since the radicals (elements) in mineral chemistry present the same general relations as those in organic, he believes there is reason for bringing the two branches more closely together than is usually done. We can decompose the latter, and there is no proof that we may not decompose the former. The following are the conclusions in his memoir which will soon be published.

(1.) The compounds which the three kingdoms offer for our study, are reduced by analysis to a certain number of radicals which may be grouped in natural families. (2.) The characters of these families show incontestible analogies. (3.) But the radicals of mineral chemistry differ from the others in this, that if they are compound, they have a degree of stability so great that no known forces are capable of producing decomposition. (4.) The analogy authorizes the enquiry whether the former may not be compound as well as the latter. (5.) It is necessary to add that the analogy gives us no light as to the means of causing this decomposition, and if ever to be realized, it will be by methods or forces yet unsuspected.

Spontaneous generation.—Mr. Pouchet, Professor of Zoology at Rouen, is a decided believer in spontaneous generation, and has undertaken to revive this theory which has been overturned by the experiments of Schultze and Schwann, as well also by those of Messrs. Schroeder and Dusch. The former have shown that animal substances do not ferment when they are enclosed in air which has previously traversed a red hot tube. The latter state that these same substances may be preserved indefinitely in air which has previously been made to pass through a tube containing cotton. They have alike concluded from their experiments that fermentation and putrefaction are only the results of the life of certain inferior animalcules whose germs were in the atmosphere, and which are developed at the expense of the fermentible substance. Remove these germs either by filtration or calcination and there is no fermentation or putrefaction.

The mechanical theory of Liebig, which has at least as much probability in its favor as the physiological just mentioned, opens a way of explanation independent both of this and spontaneous generation.

The work of Mr. Pouchet was some time since announced, and the great success of the naturalist of Rouen proclaimed. It was stated that

he would surprise the scientific world with a prodigious number of experiments directly opposed to all received ideas.

The following are the results announced. He has seen cryptogams and animalcules to be produced in vessels when every organic germ has been previously destroyed and when the air had been washed in sulphuric acid or had traversed a tube of porcelain heated to a red heat. He has even succeeded in developing organic beings in artificial air and also in pure oxygen.

The details of one of his experiments are as follows. A flask, holding a litre, was filled with boiling water, then hermetically sealed with the greatest precaution, then inverted in a mercury trough; then when the water is cold, it was opened under the mercury and half a litre of pure oxygen introduced. Immediately after was added to it, under the mercury, a small box of hay weighing 10 grams, which had just been raised, in a flask, by means of a stove to a temperature of 100° C. and kept at this temperature for 30 minutes. The flask was then hermetically sealed by the aid of its stopper ground with emery; and to make it sure, when taken from the mercury a coat of varnish and vermilion was put over the aperture.

Eight days afterward, small globules were found in the liquid and on the hay. On opening the flask at the end of ten days, the oxygen appeared to have remained pure. The whitish globules were due to a fungus in tufts which Mr. Montagne, the micrographer, called *Aspergillus Pouchetii.*

A plant is thus developed in a medium from which it was endeavored to exclude every species of organic germ; but the conclusion of Mr. Pouchet is quite too general, as no facts prove that every kind of vitality was destroyed during the exposure of the hay for 30 minutes to 100° C.

Ozonometry in the Crimea.—During the Crimean war, the French army physicians, established three observatories for ozonometric, thermometric and other meteorological observations, morning and evening each day, and also for keeping statistics of diseases and deaths. Dr. Berigny of Versailles has in charge a reduction of the observations, and the following are his conclusions on the subject of ozone.

(1.) The more the ozonometric test papers were colored in the open air, the more numerous were the sick that were taken to each of the hospitals. One of these hospitals was situated at the general quarters at Sebastopol (Observatory No. 1), the second at the south border of the Inkerman plateau (Obs. No. 2).

(2.) The higher the temperature the smaller the number of sick entered and also of deaths.

(3.) At the three observatories, the ozone curve was essentially the same; and (4.) the same was true for the temperature.

(5.) At observatory No. 1, the less the ozone, the greater the number of deaths, whilst at observatory No. 2 it was the reverse.

This is almost the only positive result which science and humanity have derived from that destructive war, which has cost so much money and so many lives.

Dynamoscopy.—Dynamoscopy is a new mode of auscultation, directed towards the examination of sounds hitherto not studied. The author,

Dr. Collongues, examines these sounds in case of a deceased person with an instrument with one extremity on the part to be ausculted and the other at the ear. It is with this instrument that Dr. Collongues supposes he is able to detect the evidence of actual death.

He found this evidence in 1854 in the case of a woman attacked with cholera who was not believed to be dead. Examining about the heart with his dynamoscope, he distinguished a crackling sound which continued even to the tenth hour after death. He followed up this trial with others, and has arrived at the following conclusions respecting this sound.

(1.) After the respiration and the beating of the heart have ceased at death, a crackling sound may be heard which he calls "bourdonnement."

(2.) The sound continues from five to ten hours after death.

(3.) It goes on decreasing from the time of death, and is last perceived about the præcordial and epigastric regions.

The results have been confirmed by observations on animals. It hence results that life continues until the cessation of this sound has ceased, and the cessation is a positive sign of death. This observation offers a means of distinguishing *lethargy* from death, as the sound does not cease in lethargy.

On applying the instrument to the extremity of the fingers, a sound of similar kind is heard which varies with the age, sex, state of health, activity or repose. The crackling is more rapid in children than in adults, and still more so than in aged persons. It is more gentle in woman than in man of the same age, and the crackling sounds ("pétillemens") are in general twice as numerous as those of man. There is also a great difference for different temperaments, and for different seasons and climates.

A singular experiment made with the instrument is to hear a faint and agreeable harmony which is made at the extremity of the fingers of a man asleep, whilst when awake there is only a great discordance in the "bourdonnement." Dr. Collongues supposes that these sounds have their seat in the nerves.

Artificial Caoutchouc.—This substance is obtained by the action of chlorid of sulphur on oils. On adding to oils an excess of this chlorid, they become heated and take a consistence more or less viscous; after some days they harden and become friable. With one part of the chlorid to nine of the oil, there is a lively reaction, chlorhydric acid is disengaged, and it is all changed into an elastic substance like sponge which whitens in the water. The products are insoluble in water, alcohol, ether, oils, and sulphuret of carbon; they are attacked neither by ammonia nor by dilute acids, and are not altered at 150° C.

These facts have been recently communicated to the French Academy and they were regarded as new. They were known by myself in 1848, and the following is a brief statement of the remarks which I have made on the subject to the Academy. "In order to protect the glass stopper of a flask containing chlorid of sulphur from incrustation, I put on it a little oil. I was not a little surprised on the next day to find the coating completely solidified. I soon recognized that the solidification had been caused by the chlorid, and that in general, this compound hardened fatty bodies by modifying them more less. Being then engaged in other

researches* I proposed to myself to take up this chance observation at another time, when I learned by a number of Dingler's Polytechnic Journal for 1849 that the fact had also been observed by Mr. Rochleder. The subject having thus lost for me its special interest, I published it without making any claims of priority.

"Since then, this observation has been taken up by Mr. Gaumond, who, by mixing the chlorid of sulphur with the sulphuret of carbon has made of it several interesting applications. He forms a soft elastic paste, with which he prepares the ink-rollers of printing presses. This was in 1852."

At the same session, Mr. Balard communicated the results obtained in his laboratory by a workman, Mr. Perra, temporarily engaged there; this was in 1853. The following are some of the results:

100 parts of linseed oil with 15 to 20 p. c. of chlorid of sulphur gave an elastic product. With 5 p. c. of the chlorid the oil was thickened strongly without hardening; in this state it is soluble in all liquids which dissolve the oils, which is not true of the other products obtained with the chlorid. On mixing a given weight of linseed oil with thirty to forty times its weight of sulphuret of carbon, and then introducing one-fourth of chlorid of sulphur, the product remains liquid for several days. On applying this liquid to glass, wood, or any other solid body, the sulphuret of carbon evaporates immediately and the solid body is found to be covered with a varnish.

The chlorid saturated with sulphur is preferable to the pure chlorid. To succeed in these mixtures, it is necessary to put the chlorid quickly into the oil, and agitate it, in order to obtain a uniform product. By degrees it becomes heated; the oil hardens more or less according to the proportions of the chlorid of sulphur. It is necessary to operate with only small amounts of chlorid and avoid the elevation of the temperature. When the mixture is perfect, the material is thrown on a polished surface, as a plate of glass: after some minutes, it is done. A corner of the pellicle is detached with the point of a knife and immediately the whole is easily removed. Several layers may be added to one another, moisture being avoided at the moment of the operation.

Mr. Perra has in this way made small boxes and the handles of knives. By inserting a metallic cloth between two plates of this hardened oil, he has obtained plates that were quite durable. With some precautions, the plates may be made transparent and unalterable in the air; for this end, it is only needed to place them in a stove in order to expel the excess of chlorid. Cold renders the products hard and brittle.

Mr. Perra has not succeeded in making a substance analogous to hardened caoutchouc. He has colored the material or veined it like marble; and for this purpose the coloring material is mixed with the oil before the chlorid is added.

These products made from vulcanized oil are inconvenient for use, as they retain for a long time a disagreeable odor. They are not acted upon by dilute acids or alkalies. In the concentrated state, the alkalies sapo-

* On the cause of the variation of angles in crystals, and on the isomorphism of homologous compounds. See Comp. Rend. Acad., 1848, and Comp. Rend. Trav. Ch. de Laurent et Gerhardt, 1849.

rify them. At 120° C. they become brown, and at a higher temperature they melt with a black color. For moulding, the material is excellent. It is in a high degree electric, and may be used for making electrical plates or the electrophorus. It readily destroys any tissues to which it may be applied.

Bromid of sulphur has properties analogous to those of the chlorid.

Photochemical experiments.—We have more than once had occasion to speak of Niepce de St. Victor, of the military ranks, who employs his leisure in the useful arts. If it shall be demonstrated that there is a fluid analogous to that of caloric and light, presiding especially over chemical phenomena, Niepce de St. Victor will have had a prominent part in the discovery. But a few months since, he ascertained the fundamental fact that a body which had been exposed to solar radiation could act in the dark at a distance on certain bodies, like light which emanated directly from the sun. The observations were made mostly with a cylinder of white pasteboard. Mr. Niepce has just noticed that the pasteboard that has been exposed to the sun, and then has been preserved in the dark in a cylinder of sheet tin (tinned iron), is still active six months afterward. This action of the chemical fluid calls to mind radiant heat.

Nitrate of uranium has in a high degree the property of magazining the chemical fluid. On exposing to the sun, under a photographic proof, paper impregnated with nitrate of uranium, and then at the end of a quarter of an hour, plunging it into a solution of nitrate of silver in the dark, a positive image immediately appears having the usual maroon tint. To fix it, it is only necessary to wash it with pure water. If the nitrate of silver is replaced by chlorid of gold, the image appears of a deep blue. These pictures resist the action of the cyanid of potassium, even on ebullition; they are therefore far more stable than photographs taken in the ordinary way.

Tartaric acid has this same property, although in a less degree. Heat increases the sensibility of the reaction. For on covering with a plate of iron heated to 50° C. both the pasteboard which bears the impression from the sun and the leaf of sensitive paper prepared with chlorid of silver, the image will appear at the end of a few minutes, while at 0° C. it requires several hours to obtain a faint impression.

One general result of the researches of Mr. Niepce is this, that the bodies which preserve the greatest activity with a dose of the sun are, with the exception of the salts of uranium, those which are the least disposed to fluorescence.

This chemical activity which certain bodies may contract under the influence of the sun's rays or *insolation*, is greater or less according to the nature of the substance; it has its limits; when a substance has reached its maximum of activity continued exposure does not add anything to it.

Paper prepared with the nitrate of uranium changes color in the light and becomes insoluble; in the dark it is decolorized, and it becomes soluble after some hours, to be colored again in the light. It reduces the salts of gold and silver, so much as to become colored and insoluble.

A body rendered active by the sun will transmit this activity by contact in the dark to another body—tartaric acid, for example.

Mr. Niepce proposes to investigate whether the permanent activity communicated to a body by the solar rays is capable of determining the combination of chlorine and hydrogen, and whether it can be acquired in a luminous vacuum. An engraving wet and subjected to the sun reproduces itself on sensitive paper. But if it is covered with some millimeters of water, the effect fails, even with a solution of a salt of uranium or tartaric acid.

After having shown that certain bodies acquire by exposure to the sun, the property of reducing in the dark, salts of gold and silver, Mr. Niepce observes further that the reduction does not take place without the intervention of an organic substance. Paper is very good for this purpose, while no action is obtained if we take, for example the edge of a porcelain plate which has just been broken; on impregnating this edge with a solution of nitrate of uranium, no effect is obtained in the sun; but there is an action if we put on the edge a solution of nitrate of silver, containing a little starch or gum, and then sulphate of iron or gallic acid; a coloration is seen in the part subjected to the sun; it is the same if silver be used in place of uranium.

The reagents which Mr. Niepce employs by preference for demonstrating this action of the light are the salts of gold and silver, tinctures of litmus and turmeric, iodid of potassium for paper prepared with starch. In many substances that have been exposed to the sun the activity communicated is apparent in the insolubility; it is on a similar principle acquired under the sun's action by gelatine containing bichromate of potash, that Mr. Talbot has founded his photoglyphy. Heat and humidity promptly cause the loss of this property.

Mr. Niepce cites many examples in which the same results are obtained on inverting the course of operations; thus, a leaf of paper impregnated with gallic acid and exposed to the sun, treated by iodid of potassium, gives a feeble image which becomes very decided if subjected to nitrate of silver. A sheet of paper impregnated with chlorid of mercury and exposed to the sun gives an image with chlorid of tin, chlorid of sodium, soda, potash, and sulphuret of sodium. In the same manner a sheet impregnated with chlorid of tin, and exposed to the sun, gives an image with sulphuret of sodium, chlorid of mercury, chlorid of gold ($Cl^3\ Au^2$) and nitrate of silver. A multitude of important facts are still to be drawn from the recent works of Mr. Niepce and we shall return to them again.

Reproduction of engravings by means of Phosphorus.—The engraving is exposed to the vapors of phosphorus burning slowly in the air; the black parts alone become impregnated with the vapors; it is then applied to a sheet of sensitive paper prepared with chlorid of silver; after a quarter of an hour of contact, the engraving is represented on the paper by a design formed of phosphuret of silver, which when it is sufficiently decided, resists the action of dilute chemical agents.

The best way of operating consists in placing the engraving in a box in front of a piece of pasteboard whose surface has been rubbed with a stick of phosphorus, and which covers one of the sides of the box. It is necessary to rub the pasteboard with phosphorus at each operation, because if the phosphorus becomes red phosphorus, it produces no effect.

Chemical nomenclature; making of new words.—To French men of science, or at least to those who seek to discover facts and new substances or species, it is a subject of gratulation that a French-and-Greek dictionary has recently been published. This dictionary is issued by the publishing house of Hachette. Its authors are three Greek scholars, of the highest merit, Messrs. Alexandre, Inspector general of the University, Planche and Defauconpret, Professors. There is also a complementary work—a Græco-French Dictionary by Mr. Alexandre alone.

Bibliography.—At MALLET-BACHELIER'S, Quai des Augustin, Paris. *Traité d'Optique physique*, par M. BILLET, Prof. in the Faculty of Sciences at Dijon, tome I.—This work is altogether mathematical, and one of the kind has long been needed in France. Prof. Billet, with whom the higher optics is a specialty, has here published the results of 20 years of labor. The volume has already gone into the hands of all opticians and professors of physics.

Cours de Physique de l'Ecole Polytechnique, par M. JAMIN. Tome I, with 270 figures and a steel plate.—Mr. Jamin is Professor in the Ecole Polytechnique, and in this work he presents the programme of the course of physics in this celebrated school. From the range of the work it might well be entitled a General Treatise on Physics, for not only are the different topics profoundly treated, but also experimental demonstrations come to the aid of the theoretical and mathematical.

At HACHETTE'S, Rue Pierre Sarazin, Paris.

Résistance des Matériaux, 1 vol. in 8vo., 2d ed.—The first edition of this work appeared in 1853, and has been rapidly exhausted. Before preparing the second, its author, General MORIN, Director of the Conservatory of Arts and Trades, desired to verify by experiment the principal theories, and to this end has made many trials to test the accuracy of the hypotheses admitted in the ordinary theory with regard to the resistance of solids to flexure. He has also experimented on the resistance of sandstone to pressure, on which subject he gives an abstract of the trials made by the French engineer, Mr. Michelot, on the resistance of stones employed for construction at Paris.

Précis d'Agriculture théorique et pratique, par MM. PAYEN and RICHARD. 2 vols. in 8vo.—Among its topics, this work reviews the most recent discoveries on the principal points in the culture of land, besides being an elementary treatise on all departments of agriculture and even Zootechny, a science in which M. Richard is authority, as we had occasion to remark when announcing his "Dictionnaire raisonné d'Agriculture."

Problèmes de Mathématiques et de Physique, par M. MENU DE ST. MESMIN. 1 vol. in 8vo.—This is a volume of exercises prepared with reference to students in the Department of Engineering including Mines, Bridges, Roads, etc. The problems are followed with solutions and explanations, and are illustrated by many figures in the text.

Dictionaire Grec-Français, et Français-Grec. 2 vols., grands in 8vo.—This work is noticed above.

Dictionnaire des Cotemporains, par M. VAPPEREAU, grand in 4° de 1800 pages en 2 colonnes.—In this work, the author proposes to give a biographical notice of the most distinguished cotemporary men in all de-

partments of science, art, industry, literature, politics, and even war. This colossal work remains in the 'state of composition' from the beginning to the end, and therefore open to emendations, until the moment of publication. Supplements will be published as may be required. The whole dictionary is so put together as to admit of modifications, which must be numerous; for the good faith of the author has been more than once surprised.

This dictionary is not addressed only to the French, for men of all lands have a place in it; and the American reader will find a biographical notice of the principal men of the State, Art, Literature and Science in the United States.

Les Philosophes Français au 19e siècle, par TAINE, in 12-de 368 pages. —This work is written with much spirit, and in a style both elegant and well adapted in our view for the scientific criticism it contains.

DESCLOZIÈRES.—*Vie et Inventions de Philippe de Girard*, broch. in 12mo, avec figures.—Philippe de Girard was the inventor of the machine for spinning linen thread, and the author of many other inventions, well exhibited and appreciated in this small work.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On intermitting fluorescence.*—J. MÜLLER has observed in platinocyanid of barium a peculiar phenomenon to which he has given the name of intermitting fluorescence. When a strip of paper is washed with a solution of the salt in such a manner that on evaporation the surface appears covered with a layer of delicate green crystals and then exposed in a dark room to the spectrum produced by a flint glass prism aided by a lens of long focal distance, almost the whole portion on which the blue rays fall appears blue. In this blue portion however, three isolated green fluorescent bands appear. The middle of one of these bands corresponds to Frauenhofer's line G; the two others lie between G and F. The centres of these bands correspond to the wave lengths 0·000462^{mm}, 0·000446^{mm}, 0·00430^{mm}. From this it appears that rays of these wave-lengths produce fluorescence, while those of intermediate wave-lengths produce none. An uninterrupted green fluorescence begins at that portion of the spectrum which corresponds to a wave-length of about 0·000410^{mm}. No similar phenomenon has hitherto been observed.—*Pogg. Ann.*, civ, 649.

2. *On the increase in the resistance to electrical conduction which depends on temperature.*—CLAUSIUS has pointed out a remarkable result deducible from the experiments of Arndtsen on the resistance of metals at different temperatures. Arndtsen had arrived at the result that in the simple metals, with the exception of iron, the resistance increases uniformly with the temperature and that in the different metals the comparative increase is nearly the same. Clausius remarks that Arndtsen's numerical results may be expressed by the formula

$$w_1 = w_0 (1 + 0{\cdot}00366, t),$$

in which w_1 is the resistance at the temperature t, w_0 the resistance at

0° C. From this it would follow that the resistance of the simple metals in the solid state to electric conduction, is nearly proportional to the *absolute temperature*. The author remarks that although this conclusion is not yet fully borne out by experiment, the numbers obtained being only approximate, it may yet be of interest and serve as an inducement to new investigations.—*Pogg. Ann.* civ, 650.

3. *On the expansion of liquids heated above their boiling points.*—THILORIER found in 1835 that liquid carbonic acid between 0° and 30° C., has a mean coefficient of expansion of 0·0142, which is about four times greater than that of air and other gases. Drion has endeavored to generalize this observation, and finds, in fact, that other volatile liquids at temperatures sufficiently above their boiling points, exhibit coefficients of expansion of similar magnitude. The following are the author's results for chlorhydric ether and sulphurous acid. The coefficient of expansion of chlorhydric ether at 0° is according to Pierre 0·00157. According to Drion the apparent expansion of the same liquid is 0·00360 of its volume, at 121° C. upon the average for every degree centigrade. Between 128° and 134° the coefficient is 0·00421 of the volume at 128°, while between 144°·5 and 149°·25 the coefficient is 0·00553 of the volume at 144°·5. The mean expansion of sulphurous acid between 0° and 18° is 0·00193. Between 91° and 99°·5 the coefficient is 0·00368 of the volume at 91°; between 108°·5 and 115°·5 it is 0·00463 of the volume at 108°·5; between 116° and 122° it is 0·00533 of the volume at 116°; between 122° and 127° it is 0·0060 of the volume at 122°. From this it appears that the coefficient of expansion of chlorhydric ether becomes equal to that of a gas at a temperature of about 125° C.; that of sulphurous acid equals that of a gas at about 95° C. Above these temperatures the coefficients increase very rapidly.—*Comptes Rendus*, xlvi, 1235.

4. *On the chemical effects of electric discharges.*—PLÜCKER has published in successive parts, the results of an elaborate and very interesting investigation of electric discharges in tubes containing rarefied gases. For the details we must refer to the original papers, which do not admit of condensation, and content ourselves with giving in the author's own words, the results which are most interesting to chemists.

I. Certain gases (oxygen, chlorine, bromine and vapor of iodine,) combine more or less slowly with the platinum of the negative electrode, and the resulting compounds are deposited upon the surrounding sides of the glass tube. When the gases are pure we approximate in this manner to a perfect vacuum.

II. Gases which are composed of two simple gases (vapor of water, ammonia, protoxyd of nitrogen, deutoxyd of nitrogen, nitrous acid,) are immediately separated into their components, and then remain unchanged, if they do not (as ammonia) unite with the platinum. If one of the gases be oxygen (as in steam and the different oxyds of nitrogen) this gradually disappears and only the other gas remains.

III. When the gases are composed of oxygen and a solid simple substance, complete decomposition by the current takes place but slowly, the oxygen going to the platinum of the negative electrode, (sulphurous acid, carbonic oxyd, carbonic acid). Carbonic acid at first splits instantly

into the lower gaseous oxyd and into free oxygen, which combines gradually with the platinum. Carbonic oxyd gas is then slowly decomposed by the combination of its oxygen with the negative electrode. The results above mentioned, were obtained by means of the so-called Geissler's tubes, which are simply glass tubes of various forms containing rarefied gases, and provided with platinum wires fused into the glass. The electric currents were partly derived from the electric machine, and partly from Ruhmkorff's apparatus. Finally, the results themselves are directly deduced from the prismatic analysis of the light of the simple and compound gases, the spectrum obtained being simple, or composed of two distinct and superposed spectra, according as the discharge passes through a simple gas or a mixture of two.—*Pogg. Ann.*, cv, 67. W. G.

5. *On a new Law of Binocular Vision;* by the Rev. J. DINGLE, (Proc. Brit. Assoc., Ath., No. 1615).—The object of the law in question is to obviate the imperfect vision which would sometimes arise from the difference of the pictures in the two eyes. In some cases this difference would lead to great inconvenience and confusion. It sometimes happens, for instance, that in looking at a field of view at some distance, objects considerably nearer are so interposed as to present themselves in the picture formed in one eye and not in the other. Thus, in looking at a landscape, if the finger or any other object is held before one eye, the image of it from the one retina is superposed in the *sensorium* on a part of the landscape formed in the other eye. On mere physical principles, this might be expected to blot out or greatly confuse that part of the landscape upon which it was placed; but upon trial this is not found to be the case, as that part is merely a little dimmer than the rest from being seen only with one eye, but is equally distinct and as truly colored.

By various experiments the author had ascertained that this was the result of a peculiar power of the will, by means of which the mind is enabled, when two different images are superposed in the *sensorium*, to select whichever it pleases, to bring that object into view, and entirely to obliterate the other,—it sees, in fact, whichever it wills to see, and the other image, simply by being neglected, becomes invisible. In ordinary vision, the determination of the image to be seen is effected by the same act of the will which determines the position of the optic axes; but by certain arrangements which were indicated both images may be made to have the same relation to the optic axes; and as the predisposition to select one or the other is thus obviated, it is made indifferent to the mind which of the two images that occupy the same place in the *sensorium* it shall see. When these arrangements are made, it is found that mere efforts of the will can easily bring either the one or the other into view. The importance of this law, which enables the mind to select its image, was pointed out in different cases of ordinary vision. It obviates the difficulty already adverted to, of having two different pictures on the same spot; it has not improbably an important influence in producing the general stereoscopic effect; it also, to some extent, remedies the effect of squinting, by obliterating the picture in the imperfect eye, which could not be else done without shutting it. The effect of the law, in some extraordinary cases, was also noticed, especially in the power of the will to fix images on the sight, as Sir Isaac Newton instances in his own case

(see his life by Sir David Brewster). The author pointed out the great interest of the subject, not only in its practical aspect, but also as having an important bearing on the connexion between mind and matter.

Prof. Stevelly said that in reference to these permanent impressions on the retina so well described in the very interesting letter from Sir Isaac Newton which had been read, he wished to mention a circumstance which occurred to himself this summer, and which he was entirely unable to account for on any optical or physiological principle with which he was acquainted. At the close of last college session he had been in weak health, and had gone out to his brother-in-law's seat in the country for a few weeks. While there he had become greatly interested in the economy and habits of the bees. "One morning, soon after breakfast, the servant came in to say, that one of the hives was just beginning to swarm. The morning was a beautifully clear, sunny one, and I stood gazing at the insects, as they appeared projected against the bright sky, rapidly and uneasily coursing hither and thither in most curious yet regular confusion, the drones making a humming noise much louder and sharper than the workers, from whom also they were easily distinguished by their size; but all appearing much larger in their rapid flights than their true size. In the evening as it grew dark, I again went out to see the bee-hive, into which the swarm had been collected, removed to its stand; soon after I was much surprised to see, as I thought, multitudes of large flies coursing about in the air. I mentioned it to my sister-in-law, who said I must be mistaken, as she had never seen an evening on which so few flies were abroad. Soon after, when I retired to my chamber, and knelt to my prayers before going to rest, I was surprised to see coursing back and forward, between me and the wall, what I now recognized as the swarm of bees, the drones quite easily distinguishable from the workers, and all in rapid whirling motion as in the morning. This scene continued to be present to me as long as I remained awake, and occasionally when I awoke in the night, nor had it entirely faded away by next night, although much less vivid. This was the first instance I had ever heard of moving impressions having become permanently impressed on the retina, nor can I give the slightest guess at the *modus operandi* of the nerve. Notices of fixed impressions, particularly after having been dazzled, are now common enough. The Rev. Dr. Scoresby, at the late meeting at Liverpool, had given a detailed account of some which had presented themselves to him; and a very curious one had occurred to me some years since. I was walking down the streets of Belfast with Sir John Macneill, the eminent engineer, when he said to me—'what has become of my old friend Green, who kept that shop; I see new people have got it.' Turning suddenly to look at the shop indicated, I was completely dazzled by the bright reflexion of the sun shining on the new brass-plate under the window of the shop, so that for some seconds I could see nothing. As we walked on I soon observed before me in the air the words 'J. Johnstone & Co.,' in blood-red characters, which soon, however, changed to other colors. With an exclamation of surprise I stated the fact, and we turned back to see whether or not this was really the inscription on the brass-plate, and found that it was. The optical account of this was simple enough. The retina had been partially paralyzed from

the intense light reflected from the plate, but as I had turned with pain from it instantly, the part corresponding to the black letters on the plate had escaped, and as I walked on the red strong light reflected from surrounding objects on this part became contracted with the darkness, as yet showing itself on all the surrounding parts of the disordered retina; as the retina recovered its tone other colors in succession took possession of the place which at first had been red. Sir J. Macneill then told me that when first he had gone to reside in London, a murder had been found out by a similar circumstance. The murderer, then unknown, had been dazzled by the reflexion of the sun from a bucket of water, which another man was carrying before him, and soon after seeing in the air what he took for a bucketfull of blood going before him, he was seized with such horror that he declared himself the murderer, and disclosed such facts as brought the crime home to him, so that he was convicted and executed."

II. GEOLOGY.

1. *A record of Earthquakes, kept at Hilo, Hawaii;* by S. C. LYMAN, (from a letter addressed to Dr. C. F. Winslow, and by him communicated for this Journal).—*June*, 1833. Two slight shocks during the month.

Oct. 3, 1833.—Two shocks in the night, one quite heavy.

Oct. 13.—A shock at 3 o'clock, P. M., motion quick, up and down.

Feb. 19, 1834.—At 6 P. M. there was a slight shaking of the earth which was almost instantaneously followed by a shock so heavy as to upset some things in the house, throw the cream off from milk standing in pans, and throw water out of a pitcher standing in a wash-bowl. At 9 o'clock in the evening there was again a shaking of the earth which lasted only a few seconds.

May 14.—A heavy shock between 2 and 3 P. M.

Aug. 3.—One at about 4 o'clock, A. M., so heavy as to waken people, and cause them some alarm.

March 23, 1835.—One slight shock at 9 A. M.

March 26.—Three shocks following each other in quick succession at 25 minutes past 6 o'clock A. M.

July 21.—Three shocks during the day.

Sept. 6.—One shock at 2 or 3 o'clock A. M.

June 20, 1837.—A smart shock at 20 minutes before 7 P. M.

Jan. 12, 1838.—A smart shock some time after midnight.

Jan. 29.—Three shocks in pretty quick succession a little past 10 o'clock P. M. The first two heavy, the last slight.

July 9.—A slight shock between 8 and 9 o'clock A. M.

Oct. 16.—A jar merely, accompanied with a noise resembling the discharge of distant cannon.

Nov. 5.—Three shocks during the day, one in the forenoon and two in the afternoon.

Nov. 6.—One shock, a few minutes before 10 A. M.

Nov. 7.—A smart shock at midnight, another at twenty minutes past 3, and another about 4 o'clock A. M.

Nov. 8.—Several shocks during the day, and twelve distinct ones during the night. There were frequent shocks during the day and night for

five days following, and then occasional ones during the month, none of them heavy, some of them accompanied by a rumbling sound, others by a noise like the rushing of water by a ship. There was one quite heavy shock, but I do not recollect the time.

Dec. 4.—A distinct though not heavy shock.

Dec. 9.—One shock at 40 minutes before 12 at noon.

Dec. 10.—One shock at 4 o'clock, A. M.

Dec. 12.—A severe shock at 1 o'clock P. M. Stone walls were thrown down, and plastering a good deal cracked, but no other damage done.

April 7, 1839.—A pretty smart shock at midday.

Feb. 1, 1840.—A severe shock at half past 1 P. M.

May 5.—A slight shock at 4 P. M.

Sept. —.—A shock at 10 P. M.

Oct. 14.—A slight shock between 8 and 9 o'clock P. M.

Dec. 18.—Two smart shocks about 5 A. M., waking us from our slumbers.

March 11, 1841.—Two shocks, one at fifteen minutes before 1, the other at 20 minutes before 2 P. M. The motion was up and down, gentle and easy.

April 5.—A smart shock at 15 minutes past 1 P. M., undulations north and south.

April 7.—At 11¾ P. M. This was the most severe shock we have ever felt. Stone walls thrown down, plastering cracked, and chimney also, milk was thrown out of the pans and water out of a pail a little more than half full. Motion undulating north and south. In just one hour there was a trembling of the earth, and after another hour another jar.

May 28.—A shock at half past 7 P. M., not heavy.

June 26.—A shock in the night.

July 3.—A shock between 5 and 6 A. M.

Sept. —.—A smart shock in the night.

Nov. 28.—A shock in the night.

Feb. 14, 1842.—A shock at 5 o'clock A. M.

May 15.—A shock in the night.

Aug. 31.—A shock at half past 9 P. M., not heavy.

Nov. 9.—A shock in the night so heavy as to awaken us.

March 8, 1843.—A shock at 7 o'clock P. M.

April 27.—A shock in the night.

July —.—Two shocks during the month. One in the night, or P. M.

Dec. 15.—A shock at 10 A. M.

Feb. 18, 1844.—A severe shock at half past 6 P. M.

Sept. 1.—A heavy shock between 7 and 8 A. M.

Dec. 21, 1845.—A moderate shock in the night.

Feb. 14, 1846.—A slight shock at 7 A. M.

June 15.—A moderate shock in the night.

June 24.—Ditto.

March 29, 1847.—A shock between 9 and 10 A. M.

Oct. 4.—A heavy shock about 3 o'clock P. M.

Feb. —, 1848.—A heavy shock at 5 o'clock P. M.

April 19.—Two slight shocks about 8 P. M.

July 9.—A severe and protracted shock at 4¾ A. M.

Dec. 5.—A slight shock at 8¼ o'clock A. M.

In 1849, none.

In 1850, only two shocks during the year.

Jan. 12, 1851.—A smart shock at 7 P. M. Premonitory shock, very distinct.

May 4.—A moderate shock after midnight.

May 11.—A slight shock at 2 o'clock A. M.

July 14.—A heavy shock at 20 minutes past 10 A. M.

Aug. 21.—A moderate shock towards morning.

March 31, 1852.—A very severe shock at 1¼ P. M.

Oct. 19.—A smart shock at 4¾ A. M.

March 2, 1853.—A smart shock at 5 o'clock P. M.

March 8.—A slight shock.

March 11.—A smart shock at 4½ o'clock A. M.

Aug. 26, 1854.—A smart shock at 4 o'clock P. M.

Oct. 29.—A smart shock at about 8 P. M. Premonitory, distinct.

March 18, 1855.—A smart shock at 8½ P. M. Vibrations continuing several seconds.

May 24.—A protracted tolerably smart shock at about 9 A. M.

June —.—A smart shock at 4½ P. M.

Aug. 3.—A tolerably smart shock at 8½ P. M. Motion up and down.

Sept. 17.—A shock at 8 A. M.

Nov. 2.—A smart shock at 7 A. M.

Jan. 8, 1856.—A smart shock at 4 o'clock A. M.

July 8, 1857.—A slight shock at 5½ o'clock, A. M.

July 30.—A severe shock at 1 A. M. Motion undulatory.

Aug. 30.—A slight shock at 1½ P. M.

Sept. 9.—A short smart shock at 9 o'clock A. M.

March, 1858.—One slight shock, time not recorded.

April —.—A protracted and rather heavy shock in the night.

June 8.—A slight shock at 3 P. M.

July 5.—A smart shock or jar at 2 A. M.

This record has been kept simply for my own gratification, and consequently is not in as good a form as it might have been could I have known that it would ever have been needed for scientific purposes. As a general thing I noted down the earthquakes as they occurred. When they have come in the night I have not always known the precise hour, and when I have been sick the day of the month has sometimes been forgotten. This must account for the imperfect manner in which my record has been kept. I shall endeavor to be more particular in future.

I would here remark that our earthquakes are never very protracted, seldom lasting over a few seconds, and they are seldom violent, though generally sufficiently so to shake our nerves.

Hilo, Hawaii, Sept. 11, 1858.

2. *Hadrosaurus Foulkii, a new Saurian from the Cretaceous of New Jersey, related to the Iguanodon*, (from the Proceedings of the Acad. Nat. Sci., Philad., 1858, p. 213.)—Mr. Wm. Parker Foulke made a statement respecting the fossil bones, shells and wood presented by him to the Academy this evening.

Passing the summer and autumn at Haddonfield, Camden County, New Jersey, Mr. Foulke learned that one of his neighbors, Mr. John E.

Hopkins, while digging marl upon his farm, about twenty years ago, had found some bones. These were described as vertebræ, and as being of large size, and very numerous. Mr. Hopkins being young at the time of the discovery, and not specially interested in such subjects had permitted visitors to carry away the fossils; so that none remained in his own possession, nor could he remember the names of any of the persons by whom the vertebræ had been taken. According to his recollection, no head had been found, nor any other bones than those of the spine, except one, which was said by him to have resembled in general respects, a "shoulder blade." It appeared then, not improbable that upon digging around the old pit, (which was sixteen feet long and eight feet wide,) a head, or at least a portion of one containing teeth, might be obtained. Considering the geological age of the formation upon which Haddonfield stands, and that specimens of Mosasaurus had been discovered in places not very remote from the village, there appeared sufficient motive for exploration. Mr. Hopkins with an intelligent appreciation of the object proposed, gave to Mr. Foulke, with prompt liberality, permission to dig in any part of the farm, and to take away whatever fossils might be thus procured. There was some difficulty in ascertaining the place of the old excavation. It had been made in the bed of a narrow ravine, in which a brook flows eastwardly into the south branch of Cooper's Creek; but the pit had long since been filled to the common level of the bed, and it was in like manner overgrown with grass, shrubs, and young trees, so as to be undistinguishable by the eye. After conference with one of the diggers who had been employed at the time of the discovery, (whose indication proved to be inaccurate,) and after a careful survey of the vicinage by Mr. Hopkins, a party of experienced marl diggers were set at work; and after one day's preliminary trial, the eastern side of the old pit was detected. In conformity with Mr. Hopkins's recollection of the manner in which the vertebræ lay, the party of diggers was shifted to the western side of the old pit. The superficial deposit overlying the marl here, was only about four feet thick; the ravine being between twenty and thirty feet deep. At nearly four feet further depth, a thin stratum of decomposed shells was passed; and at about two feet below this, overlying and intermixed with another stratum of shells, the workmen came upon a pile of bones—the same now before the Academy. The total depth from the surface was between nine and ten feet. * * *

Dr. Leidy stated that the bones, mentioned in the remarks of Mr. Foulke, obtained from the marl of Mr. Hopkins's farm, near Haddonfield, New Jersey, and now exposed to the view of the Society, were those of a huge herbivorous saurian. The animal was closely allied to the great extinct *Iguanodon* of the Wealden and Lower Greensand deposits of Europe; the genus is, however, different, and for it the name of *Hadrosaurus*, is proposed.

Besides a number of small fragments, the bones consist of twenty-eight vertebræ, mostly with their processes broken away; a humerus, a radius and an ulna, complete; an ilium and a pubic bone, imperfect; a femur, a tibia and a fibula; two metatarsal bones and a first phalanx, complete. There are also in the collection nine teeth and a small fragment of the lower jaw.

The bones are ebony black, from the infiltration of iron, and are exceedingly heavy. Their texture is firm and well preserved; and they are neither crushed nor water rolled. In association with them, besides the shells and wood, were found several teeth of *Odontaspis* and *Enchodus*.

Most of the specimens of teeth of the *Hadrosaurus* appear to have belonged to the lower jaw. These, when unworn and perfect, are about two inches long, and of all known teeth mostly resemble those of the *Iguanodon*. They have a demi-conoidal crown, with a lozenge-shaped enamel surface directed inwardly, and divided by a prominent median carina. The upper borders of this surface are provided with short, transverse, tuberculated ridges. The body of the crown outwardly is paraboloid in transverse section, and is prolonged into a laterally compressed conoidal fang. As the teeth were worn away from the summit, their gradually expanding triturating surface sloped downward and outward. This surface is shield-like in outline, is bordered by enamel internally, and crossed by a slightly elevated crucial ridge with diverging branchlets. The ridge, resulting from the latter ossification of the dental pulp, is harder than the surrounding dentine, and is adapted to retain a rough triturating surface. The sides and bottoms of the teeth exhibit the impressions of lateral and inferior successors, and appear to indicate that the teeth in use, together with those more or less developed within the jaw, had a quincuncial arrangement.

Two of the specimens of teeth perhaps belong to the upper jaw. They differ from the others in the extraordinary degree of development of the median carina of the crown. The enamelled surface was perhaps directed in a reverse manner to that of the lower teeth; that is to say, outwardly. It is likewise lozenge-like in outline, and tuberculated at the lower borders. The body of the crown inwardly is half oval in section. The fang for more than half its width is prolonged from the carina of the crown. These teeth also exhibit the impress of successors holding the same relative position with one another as in the lower teeth.

The fragment of the lower jaw is a portion of the left dentary bone, and is three inches in depth. It has an outer parapet wall about two inches high, with deep vertical grooves for the support of the teeth. No corresponding wall appears to have existed on the inner side of the latter.

The cervical vertebræ have their bodies prominently convex in front and deeply concave behind, and would appear to indicate that Mantell was correct in assigning similar vertebræ, found in the Wealden deposits of England, to the *Iguanodon*. Three cervical vertebræ, suspected to be the third, fourth, and fifth, are two and a half inches long at the sides.

Five succeeding vertebræ, not immediately conjoining the ones just mentioned, and supposed to be anterior dorsals, likewise have convexo-concave bodies. At the sides of the latter they are from 3 to 3½ inches long, and posteriorly are 3½ inches wide. The sides of their arch present a deep pit for the articulation of a rib; but no articular mark is perceptible at the sides of the bodies. Two other vertebræ, perhaps posterior dorsals, have the bodies slightly prominent in front and slightly concave behind; and they are 3½ inches long at the sides, and 4⅜ inches wide posteriorly.

The caudal vertebræ possess articular surfaces for chevron bones; and the specimens we possess, from different parts of the tail, give the following succession of measurements of their bodies: length 2½ inches, breadth 5 inches; length 3 inches, breadth 4½ inches; length 3 inches, breadth 3½ inches; length 2¾ inches, breadth 2¼ inches; length 2½ inches, breadth 1¾ inches. From the gradation of size of seventeen specimens in the collection, it may be estimated that there were originally about fifty vertebræ to the tail. This number may be too great by about ten, but certainly not more.

A caudal vertebra from near the middle of the tail has its arch and spinous process complete. The two latter together measure 11 inches long from the body, which is 4½ inches deep. The addition of a chevron bone would indicate the tail of the animal, at its middle, to have been between one and a half and two feet in depth.

The *humerus* is perfect, and is 23 inches long. Its breadth at the tuberosities, between which the head projects midway, is 7 inches. The shaft above is compressed from without inwardly; its lower part is cylindroid, and near the middle of the bone measures 9¾ inches in circumference. At the condyles the transverse diameter is 5¼ inches. Only a very short and narrow medullary cavity occupies the centre of the shaft.

Both bones of the fore-arm are solid. The *ulna* is 23 inches long, and 7 inches in circumference at the middle. The *radius* is 20 inches long, and 6 inches in circumference at the middle.

A very great disproportion exists between the bones of the fore and hinder extremities. So much is this the case, that I was at first inclined to believe they belonged to different animals. The disproportion is even greater than in the *Iguanodon*, as indicated by comparison with the remains of an individual of the latter, in the British Museum, known as the Maidstone specimen.

The *ilium* has its two extremities broken away, and in its present condition is 27 inches long. Its sacral articular surface is 12 inches long by three inches thick. The breadth of the bone, opposite the latter surface, is from 7 to 9 inches. A bone, which I suspect to be the *pubic*, but which appears to correspond with that of the Maidstone *Iguanodon* described as the clavicle, is 26 inches long in its present state; one end being broken away. The remaining pubic extremity is 10½ inches wide.

The *thigh bone* is 40 inches long; its breadth at the head and adjoining trochanter is 9 inches; its breadth at the condyles is 8 inches; and the antero-posterior diameter of the internal condyle is 10 inches. The shaft is quadrate, and provided at its middle portion internally with a large trochanter. The circumference of the shaft just above the latter is 17 inches; just below it, 15 inches. The condyles in front enclose a large foramen terminating a groove descending from the shaft. Posteriorly, at the bottom of the intervening notch, they enclose a smaller foramen. The medullary cavity is of large size, and extends about half the length of the shaft through its middle portion.

The *tibia* is 36½ inches long; its breadth at the upper part is 11 inches; and its breadth below is 10 inches. Its shaft is narrow and cylindroid at the middle, where it measures 11¾ inches in circumference.

From this position it rapidly expands towards the two extremities of the bone. The medullary cavity is very short and narrow.

The two *metatarsal bones* are of robust proportions and are each about 11 inches long. The *proximal phalanx* of a toe is 6 inches long, and 5¼ inches broad at base.

If we estimate the number of vertebræ of the trunk of *Hadrosaurus*, to have been the same as in the recent Crocodile and Iguana; the number of sacral vertebræ to have been the same as in the *Iguanodon;* and the number of caudal vertebræ to have been fifty; the whole number of vertebræ would have been eighty. A calculation of the length of the specimens of vertebræ in our possession, with a proper allowance of separation by intervertebral fibro-cartilages, and an addition of two and a half feet as an estimate of the length of the head, would give, as the total length of the animal, about *twenty-five* feet.

The great disproportion of size between the fore and back parts of the skeleton of *Hadrosaurus*, leads me to suspect that this great extinct herbivorous lizard may have been in the habit of browsing, sustaining itself, kangaroo-like, in an erect position on its back extremities and tail. As we, however, frequently observe a great disproportion between the corresponding parts of the body of recent and well known extinct saurians, without any tendency to assume such a position as that mentioned, it is not improbable that *Hadrosaurus* retained the ordinary prostrate condition, progressing in the manner which has been suspected to have been the case in the extinct batrachian of an earlier period, the *Labyrinthodon.*

Hadrosaurus was most probably amphibious; and though its remains were obtained from a marine deposit, the rarity of them in the latter leads us to suppose that those in our possession had been carried down the current of a river, upon whose banks the animal lived.

Occasionally uncharacteristic fragments of huge bones have been found in the green sand of New Jersey, (of which we have several in the collection of the Academy,) which I suspect to belong to *Hadrosaurus.* One of these specimens exposed to the view of the members, indicates a much larger individual than the one whose remains have been presented this evening.

The species I would respectfully propose to dedicate to our fellow member, W. Parker Foulke, than whom none of our number is more zealous in the advancement of the great objects of this Academy.

3. *Ichnology of New England:* A Report on the Sandstone of the Connecticut Valley, especially its Fossil Footmarks, made to the Government of the Commonwealth of Massachusetts by Edward Hitchcock, Professor in Amherst College. 220 pages 4to, with 60 quarto plates.—Professor Hitchcock has here given us a revision of his labors on the subject of the Connecticut River Footmarks, in which he has so long and successfully labored, and, besides, has added a large amount of new material and many fine plates in its illustration. The volume opens with a bibliography containing a list of all publications on American Fossil Footprints. The characters, conditions, and origin of the strata are then discussed, and following these pages, the descriptions of the various fossil imprints.

On the age of the sandstone Prof. Hitchcock concludes: that the upper half of the sandstone,—that east of the trap range of Mount Tom,

—is not older than the Lias; and that the Virginia and North Carolina beds are of equivalent age; the lower half of the same sandstone, which may be a mile in thickness, according to the measurements of Prof. H., are thick enough to embrace the Triassic and Permian; but no evidence has been obtained that the Permian is represented.

Since the discovery of the Permian in the west as a direct continuation of the Carboniferous beds, and as the closing part properly of the Carboniferous system, it has become more apparent, we think, that the beds on the Atlantic border from the Connecticut valley to North Carolina, belong to a later period. The elevation of the Appalachian mountains appears to have closed the Palæozoic era, and thus separates the Permian period, the last of the Carboniferous age, from the Triassic, the first of the Reptilian. The observations of Prof. Hitchcock tend to confirm this view, for the rocks all appear to belong to one system: the fossils of the upper half are as recent probably as Lias; and no trace of a Permian species has been found in any of the beds.

The footprints are referred to Marsupialoid animals (5 species); Birds (31 species); Ornithoid reptiles, or reptiles walking on their posterior feet (12); Lizards (17); Batrachians (16); Chelonians (8); Fishes (4); Crustaceans, Myriapods and Insects (19); Annelids (10)—in all 123 species, more than double the number announced ten years since. The reference of some of these species to the special division in which they occur is still quite doubtful, as Professor Hitchcock states, especially the Chelonian and Marsupialoid tracks. It is not possible to present the arguments respecting them satisfactorily in a brief notice, and they would be imperfectly appreciated without figures; we therefore refer our readers to the work. The question whether any of the tracks were made by birds has seriously come up, since it has been found that some species (placed among the Marsupialoids in the work, but probably Reptilian) had 3-toed bird-like hind feet, and hand-like fore feet. The descriptions of the species are given with much detail and illustrated by characteristic figures, which enable any that are interested to pursue the subject and work out their own conclusions, where those of the author are not deemed satisfactory. It is quite possible that some of the genera of reptiles are identical with those that have been made out from fossils in Europe. Much is to be learned respecting the tracks of living animals, and the variations for running, walking and standing, before the subject will be exhausted.

The Ichnological Cabinet at Amherst contains a magnificent display of specimens, and if Professor Hitchcock had done nothing more than collect this cabinet, he would have made his mark on the science of geology. In pronouncing the display magnificent we speak advisedly. The Cabinet is by no means fairly treated in a sketch on one of the plates. The hall is 100 feet long and 30 wide; and it is filled from one end to the other with slabs of various sizes, some eight feet and upwards in length. Of the huge Brontozoa and Otozoa there are many specimens; and one series of the latter of eleven tracks covers a slab 30 feet long. The hand-like hind feet of the Otozoum are 20 inches long. There are a few tracks of the fore-feet of this biped batrachian (?) which are a little less than half the length of the hind feet; they show that the ani-

mal sometimes brought its anterior limbs to the ground though generally walking on the posterior pair.

The delicate tracks of insects or crustaceans are also remarkable. There is a specimen with impressions of what appears to be a neuropterous larve, although of doubtful relations. The Cabinet contains specimens of all the species that have been discovered in the Connecticut valley. The number of tracks on all the specimens collectively is not less than 8000, averaging 68 tracks for each species.

4. *Geological Survey of Canada.* Report of Progress for 1857. 240 pp., 8vo. Toronto, 1858. Sir W. E. Logan, Geologist.—This valuable Report includes notices of the Laurentian rocks about the mouths of the French River, the Huronian and other rocks of Echo Lake, and the limestone of Bruce Mines, by A. Murray, Esq.; on the Magdalen river and Lake St. John and its deposits, by James Richardson; on the modern fauna of some localities, by R. Bell; on Canadian Graptolites, by James Hall; Palæontological Report by E. Billings; on the composition of some Dolomites, and the origin of magnesian limestone, and on Fish manure, by T. S. Hunt; and an abstract of telegraphic observations for longitude, by Lieut. E. D. Ashe, R.N., with maps, and wood-cuts illustrating the different topics.

Mr. Richardson states in his report on Lake St. John, that recent shells (Saxicava rugosa) occur on Belle river half a mile below the falls, (near lat. 48½° and long. 71½°,) at a height of probably 200 to 300 feet above the sea; also on River St. Alphonse, about four miles above its entrance into the upper part of Ha-Ha Bay, about 150 feet above the sea. The recent researches of Mr. Hall on Graptolites have already been noticed in this Journal. Mr. Billings describes new species of corals, and new genera and species of bivalves from the Silurian of Canada, illustrating several of the latter by figures; and besides he presents important comparisons between the rocks of Canada and New York. The researches of Prof. Hunt on dolomites are of much interest, and we propose to cite from them in another number.

5. *The Quarterly Journal of the Geological Society of London*, vol. xiv, Part 4, No. 56.—This new number contains the conclusion of the annual address of the President, also the important papers reviewing the Geology of the United States by Dr. J. J. Bigsby, (which, were it not for their great length, we should be pleased to reproduce in this Journal,) and a paper by Mr. H. C. Sorby on the microscopic structure of some crystals, besides other shorter papers.

We cite here the conclusions to which Dr. Bigsby has arrived, without wishing to endorse all as they stand. They relate to the central Palæozoic basin or area of Middle North America. They are in part similar to what has before been presented in this Journal and elsewhere, by other writers.

1. That, whatever may be the case elsewhere, the Silurian and Devonian systems of New York are parts of one connected and harmonious period—the product of successive and varying Neptunian agencies, operating in waters which deepened westward from the Atlantic, and southwards from the Laurentine chain on the north.

2. That from the Catskill group (Old Red Sandstone) downwards through the whole series, to the Potsdam Sandstone, there is perfect and close conformability, and no such unwonted change in fossil life as to constitute a *systematic* break, except at one place—the Oriskany Sandstone, the base of the Devonian in New York,—there being no break of like importance at the Oneida conglomerate period, contrary to an opinion towards which able geologists are now inclining,—an opinion which leads them to consider the break at the Oneida conglomerate as systematic.

3. All the palæozoic groups of New York slowly pass one into the other by gradation of mineral and organic characters, with easily explained exceptions.

4. The palæozoic strata of New York are comparatively thin. They seem to have lost in thickness what they have gained in extension.

5. De Verneuil rightly divides the New York groups into two great classes,—the "constant" and the "local." Among the former are Potsdam Sandstone, Trenton Limestone, and Niagara. Among the latter are the four lower Helderbergs, and perhaps Oneida conglomerate, &c. This is a useful division.

6. That it is both convenient and natural to divide the Silurian and Devonian systems of this State each into three stages,—the division being based on change of sediment and their fossil contents.

7. The Middle Silurian stage is a period of especial transition—from the coarseness of some of its sediments, from their innumerable and minute alternations, and from the organic poverty prevailing.

8. That the presence of Oneida conglomerate in New York does not necessitate a change of name for all the strata below it (of "Cambrian" for instance;) because a conglomerate does not always indicate *systematic* change,—not even if there be volcanic intercalation, provided there is conformableness, and some community of fossils.

The Oneida conglomerate seems to be local, is supernumerary, and only found at present on the east of Middle North America.

9. The hardening and crystallizing effect of metamorphism is seen only in the neighborhood of hypogene rocks.

10. The New York basin exhibits few uplifts, and those of limited magnitude; no uplifts dividing it into a series of deep basins contained in hypogene beds, as in Bohemia, Wales, &c. Neither has it sheets of alternating volcanic grit (conformable,) save in the Potsdam rock on Lake Superior.

This basin has a "lay" or position of its own, as a number of undulating sheets of sediment, dipping slightly to the southwest, here and there pierced by a peak of crystalline rock, and in certain regions raised into three broad low domes of great length.

11. The sedimentary rocks of this basin have submitted to two kinds of plutonic disturbance, independent of each other, and acting at distant intervals: 1st, that of secular or slow oscillation during deposition; 2nd, that of disturbance arising from paroxysmal uplifts long after their completion.

12. The whole Silurian and Devonian series of strata having, during deposition, sunk to the depth of 13,300 feet, it is submitted as a query whether it does not seem necessary to suppose that they were elevated

into their present position by the post-carboniferous uplift,—such agency being sufficient to produce all the observed phenomena, and the effects diminishing westwards from the central line of disturbance. No other agency is known to me, although hinted at by [some] American geologists.

13. It is a remarkable fact that brine-springs exist in considerable quantity in the middle stage of the Silurian system, a group or two below the Onondaga salt-springs of the upper stage, and three palæozoic systems below any salt deposits in Europe.

14. That the form and direction of the five great Canadian lakes are not due originally and mainly to the passage of loaded waters over their site, but that they follow the outcrops of their containing sedimentary rocks; changes in shape and size having, nevertheless, occurred since.

15. The contours of the valley of the St. Lawrence generally (to which much of New York belongs), and its increasing elevation southwestwards, inland from Montreal, are due to the successive altitudes assumed westward, in slopes and plateaux, by the Silurian and Devonian strata, the lowest or most ancient being on the east. This is beautifully evidenced in the rocks forming the basins of the great Canadian lakes.

16. That some of the groups, during and after deposition, were subatmospheric, presenting the conditions of dry land and shallow waters for long and varying periods,—and that, together with the marine life they supported, they enjoyed the influences of the sun and other meteorological agencies. This is indicated by animal tracks, sun-cracks on ancient shores, the short ripple-marks of a chopped sea, impressions of reeds waving in running water, and by the presence of bog-iron-ore. This is conformable with what took place in the carboniferous, permian, triassic, liassic, oolitic, wealden, and later periods. Denudations also occurred to most of the groups to a large extent.

17. That in New York, as elsewhere, there is an intimate connexion between fossils and their sediment or habitat. The calcareocolous animals are always found in limestone more or less pure, and the arenicolous in sandstone more or less pure,—with exceptions, such as usually happen with respect to locomotive animals. The calcareocolous are everywhere the most numerous. It is true that molluscs are the principal agents in the deposition of calcareous sea bottoms; but these latter greatly favor afterwards the multiplication of individuals.

18. That the iron-ore which we so frequently see investing invertebrate remains, had access to them after their death and sepulture.

19. Every group, as established by the State Geologists of New York, is a distinct centre of life,—a separate realm or community of animated beings, which may be called epochal, so marked are the differences.

The majority of these existences always perished at the end of the group when certain deposits ceased, because the new sediment, with its new and peculiar flora (and for other reasons,) was only able to nourish a few, if any, of the old molluscs.

20. In New York the species of fucoids occupy and are typical of only one group.

21. All the individual existences are perfect at once, from the earliest dawn of life, in their organization and social relations.

22. It is a great thought, that throughout the incalculably long succession of fossiliferous deposits, palæozoic or more modern, all animal and vegetable life was constructed upon the same idea of innervation, organs of sense, supply and waste, fecundation, &c.

23. There is another kind of life-centre—the geographic, belonging to one and the same group. This forms numerous separate provinces linked together by a few common fossils, and displaying extraordinary variety. This principle or regulation is carried out abundantly everywhere. Bohemia and Scandinavia have scarcely a Silurian fossil in common. One half of the Russian and Irish fossils, and two thirds of those of New York, are new and peculiar. Even the east and west sides of the small districts in Wales and England investigated by Prof. Philips, differ remarkably in their population. We see this in the American Tertiaries and in the recent seas.

24. Contrary to the opinion of Mr. D. Sharpe, the mollusc having the greatest vertical range has the greatest horizontal extension, being found in the most distant regions.

25. There is no evidence of multiplication of species by transmutation.

26. Fossils may be contemporaneous in geological age, without being contemporaneous *in time* as commonly understood.

Geological age is partly determined by fossil evidence. Now, the presence of living beings (subsequently fossil) depends on mineral and other conditions, such as temperature, depth, currents, &c., which were nowhere the same for large spaces, but were always undergoing changes from plutonic and other causes—changes always more or less local and limited, the deposits being thick or thin in places: so that the universal scheme of palæozoic life was not everywhere worked up to the same point; here preparations were making for Lower Silurian deposits,—there for the Upper, or Devonian, and so on. Thus isochronism was perhaps not common.

27. The principles of recurrency, succession, increment, and relative abundance of fossil species are the same in New York, Wales, and elsewhere, modified by local circumstances.

28. Recurrency, or reappearance in different strata, is at the same time the measure of viability in the species, and of connexion in the groups of strata. It is a kind of living nexus, pointing out that the groups belong to one and the same order of things. It may have been partly caused by migration.

Recurrency is not so common in New York as in Wales,—in other words, vertical range is longer in Wales. Great depth is an obstacle to the existence or transmission of living creatures.

29. Everywhere, on the eastern as well as on the western continent, the same fossils, of all orders and kinds, appear in the same succession. A very few Crustacea and a *Lingula* or *Obolus* or two, amid a dense matting of fucoids, appear at what now seems to be the dawn of life; then some Gasteropoda, a few Cephalopoda, and a few Brachiopoda in the third group from below (Chazy). But in the fifth group from below (Trenton,) multitudes of Zoophyta, Bryozoa, Brachiopoda (save *Spiriferi*,) Orthocerata, and Trilobites spring forth; but not a Lamellibranchiate. As species, they nearly all perish with the advent of a new deposit; but,

as genera, they appear one after another through the successive epochal centres, becoming multiplied in numbers and perfect in form. Then they lessen in numbers, dwindle in size, and finally disappear.

30. There is a close similarity in New York and Wales in the increment and decrement of Zoophyta, Bryozoa, Echinodermata, Brachiopoda, &c.; that is, these fossils are numerous and few at the same points of the Silurian scale.

31. The same genera, species, and amount of individuals abound or are few in the countries just named. Brachiopoda, Crustacea, Orthocerata, are many; Lamellibranchiates few. The extraordinary opulence in fossils of the Rhenish Devonian strata does not obtain in New York. In New York, however, according to our present list, the Lower Silurian stage is the most fossiliferous; in Wales, it is the Upper. Future discoveries may change this condition of things.

32. A remarkable feature in the uppermost four groups of New York Siluria (the Lower Helderberg) is the substitution in them of limestone for the arenaceous mud of the Welsh Ludlows, their contemporaries. It has given them a Wenlock character. But it is to be remembered that the Ludlow and Wenlock groups of Wales are in close fossil connexion, —74 out of 311 species of organic remains being common to both, or very nearly one quarter.

I shall not proceed at present with these inferences into the American Devonian system, although there is no want of interest. I may just remark that many Silurian Brachiopoda and some other molluscs work themselves up into the Devonian as representatives of a common period. They may even be found in the Carboniferous system, as has been proved by D'Archiac and De Verneuil to be not uncommonly the case in Europe.

The great ruling zoological principles of the Silurian system are continued into the Devonian; but in the latter we have the introduction of Vertebrates in profuse variety, and of new and complex types of Invertebrates in unwonted abundance, the old forms dying out.

6. *Report of the State House Artesian Well at Columbus, Ohio;* by W. W. Mather. 42 pp., 8vo., Columbus, 1859.—The Artesian well at Columbus had reached a depth of 1858 feet early in last December. For the first 23 feet the material passed through was sand, clay and gravel; then 15 of slate and 14 feet of Columbus limestone referred to the Devonian; 115½ feet Columbus limestone, probably Upper Silurian; below this 277 feet, the blue limestone of Cincinnati; then 187 feet (or to a depth of 764 feet,) limestone shales with salt water at 675 feet; then 823 feet of greenish marley slates, probably equivalent to the Utica slates of New York. Prof. Mather observes that "if the Cincinnati or Blue limestone be the equivalent of the Trenton limestone, Utica slates and Hudson river group, there must be a great depth of mud rock in Ohio, of which no traces exist in New York, Pennsylvania, or other States around us," and he inclines to regard the Blue limestone as Upper Silurian, but without settling the question by a sufficient appeal to facts.

7. *Synopsis des Echinides Fossiles;* par E. Desor. 1 vol. text, and 490 pp. 8vo, with 1 vol. of 44 8vo plates. Paris and Wiesbaden, 1858. —This work by Mr. Desor, now of Neufchatel, is devoted to the fossil Echinidæ, and contains descriptions of 1415 species of all geological ages, with a very large number of fine figures on the 44 crowded plates.

III. BOTANY AND ZOOLOGY.

1. *British National Museums of Natural History.*—The separation of the Natural History collections at the British Museum from the library and antiquities, seems to be inevitable at no distant period. An influential memorial, addressed by leading naturalists to the Chancellor of the Exchequer, strongly recommends this separation to be effected at the present time; and it is thought that the recommendation will be adopted. It is proposed to establish separate museums of zoology, botany, and mineralogy, and even to divide each of these into a *typical* or *popular*, an *economic*, and a *scientific* department, the two former open always to the public, the latter to men of science. The zoological and perhaps the mineralogical collections it is proposed to concentrate in some part of London or the immediate vicinity, probably at Kensington Gore; the botanical collection would of course go to Kew, where the national botanical garden,—brought to a high state of perfection under Sir Wm. Hooker's superintendence,—and a large museum of economic botany, most successfully established by him, already exist. There is likewise an excellent herbarium, which was some years ago presented to the establishment by Mr. Bentham. There also is the great herbarium of Sir Wm. Hooker, perhaps the largest in the world,—certainly far larger than any other ever formed in one life time, or by a private person, and the one which for the last twenty years has contributed more than any other to the advancement of the science. For almost 20 years the Hookerian herbarium —even more than the Royal Gardens—has made Kew the head-quarters of botany, rivalling the imperial establishment on the other side of the channel, and more useful as well as more freely accessible to botanists from every part of the world than the national herbaria at the British Museum. As to accessibility, indeed, no fault is to be found with the latter; the Banksian and other herbaria of historical importance could always be consulted under proper regulations. But it must be said that, with the greatest botanist of the age as their curator, these national collections for a quarter of a century have not contributed to the advancement of botany to any thing like the extent which the Hookerian herbarium, and its devoted, generous-spirited, and disinterested founder have done. How such a vast herbarium can have been collected and maintained, in perfect working order, by a private individual of very moderate means, it is not easy to conceive. Certainly it is too large and too important for science long to remain in private hands. It must in any case be acquired by the British Government; when this and the Benthamian herbarium, with adequate provision for their increase,—supplemented by the Banksian and other special collections now at the British museum (which should be kept distinct)—will form an unrivalled *scientific botanical museum.* A. G.

2. *On the Coiling of Tendrils;* by Prof. Gray.—As much as twenty years ago, Mohl suggested that the coiling of tendrils 'resulted from an *irritability* excited by contact.' In 1850 he remarked that this view has had no particular approval to boast of, yet that nothing better has been put in its place. And in another paragraph of his admirable little treatise on the Vegetable Cell (contributed to Wagner's *Cyclopædia of Physiology*), he briefly says: 'In my opinion, a dull irritability exists in the

stems of twining plants and in tendrils.' In other words, he suggests that the phenomenon is of the same nature, and owns the same cause (whatever that may be) as the closing of the leaves of the Sensitive-plant at the touch, and a variety of similar movements observed in plants. The object of this note is to remark that the correctness of this view may be readily demonstrated.

For the tendrils in several common plants will coil up more or less promptly after being touched, or brought with a slight force into contact with a foreign body, and in some plants the movement of coiling is rapid enough to be directly seen by the eye; indeed, is considerably quicker than is needful for being visible. And, to complete the parallel, as the leaves of the Sensitive-plant, and the like, after closing by irritation, resume after a while their ordinary expanded position, so the tendrils, in two species of the *Cucurbitaceæ*, or Squash family, experimented upon, after coiling in consequence of a touch, will uncoil into a straight position in the course of an hour; then they will coil up at a second touch, often more quickly than before; and this may be repeated three or four times in the course of six or seven hours.

My cursory observations have been principally made upon the Bur-Cucumber (*Sicyos angulatus*). To see the movement well, full-grown and outstretched tendrils, which have not reached any support, should be selected, and a warm day; 77° Fahr. is high enough.

A tendril which was straight, except a slight hook at the tip, on being gently touched once or twice with a piece of wood on the upper side, coiled at the end into 2½–3 turns within a minute and a half. The motion began after an interval of several seconds, and fully half of the coiling was quick enough to be very distinctly seen. After a little more than an hour had elapsed, it was found to be straight again. The contact was repeated, timing the result by the second-hand of a watch. The coiling began within four seconds, and made one circle and a quarter in about four seconds. It had straightened again in an hour and five minutes (perhaps sooner, but it was then observed); and it coiled the third time on being touched rather firmly, but not so quickly as before, viz. 1¼ turns in half a minute. I have indications of the same movement in the tendrils of the grape-vine; but a favorable day has not occurred for the experiment since my attention was accidentally directed to the subject. I have reason to think that the movement is caused by a contraction of the cells on the concave side of the coil, but I have not had an opportunity for making a decisive experiment.—*Extr. from Proceedings of the American Academy of Arts and Sciences*, vol. iv, p. 98, *Aug.* 1858.

3. *An Essay on the Tape Worms of Man*, giving a full account of their Nature, Organization, and embryonic development, the pathological symptoms they produce, and the remedies which have proved successful in modern practice, by D. F. Weinland, Ph.D.,—to which is added an Appendix containing a catalogue of all species of Helminths hitherto found in man. 94 pp., 8vo, illustrated with original wood cuts. Cambridge, Mass., 1858. Metcalf & Co.—Dr. Weinland is high authority on all subjects connected with Intestinal worms, and especially the species that infest man, of which 32 are now known. This pamphlet is valuable both pathologically and zoologically. Nothing in the whole range of

animal life is more strange than the history of the tapeworm, and as the facts have not been in this Journal, we cite a few paragraphs on the subject.

"Every butcher is acquainted with the disease in the muscles of the domesticated hog, denominated 'measles,' and calls the flesh of such a hog 'measly pork.' It has long been known that those pea-like whitish globules (measles) contain a curious animal, namely, the perfect head and neck of a tapeworm, ending however, not in the long, jointed body of the regular tapeworm, but in a water-bladder. No traces of reproductive organs are to be seen. Such measles are found not only in the hog, but also in other animals, where they are better known under the name of *Hydatids*. For example, they are very often met with in the liver of rats and mice; in the mesentery of the hare; and even, though more rarely, in the muscles of man; and those of the latter have turned out to be of the same species (*Cysticercus Cellulosæ*, Rudolphi) as those found in the hog. All the different species of this sort of hydatids are known in science under the generic name of *Cysticercus*.

Again, other hydatids, varying from the size of a pea to a diameter of several inches, are occasionally found in the lungs, the liver, and other organs of man, but more frequently in the liver and lungs of our domesticated Ruminants, such as oxen, sheep, and goats. These hydatids are roundish bladders of a milky-white color, containing a watery fluid, in which swim many whitish granules; each of these granules is, as a good lens will show, a well-developed head and neck of a Tænia, inverted into a little bag. This kind of hydatid, also, has been considered as a distinct genus of intestinal worms, and called *Echinococcus*.

Again, a disease frequently occurs in the brain of sheep, producing vertigo (German, *Dreher*, French, *tournis*). This was ascertained, years ago, to be caused by another sort of hydatid, appearing as a bladder, often of several inches in diameter; and, as in Cysticercus and Echinococcus, filled with a watery fluid. On the outside of these bladders are attached a number (often hundreds) of tapeworm heads, all retractile into the inside of the bladder by inversion like the finger of a glove. This hydatid was considered by zoologists as a third genus, called *Cœnurus*.

These three genera, *Cysticercus*, *Echinococcus*, and *Cœnurus*, formed until recently an order in the class of intestinal worms, called *Cystica* (Bladder worms, or Vesicular Worms). But *we now know that all of this group are merely larves of tapeworms*, and that the whole order of *Cystica*, being composed of larves of *Cestoidea*, must therefore be dropped from our zoological system.

This important discovery was made as follows. Ephraim Götze, a German clergyman and naturalist of the last century, had noticed a singular similarity between the heads of some Cysticerci and those of some tapeworms. He had particularly noticed this similarity between the tapeworm of the cat (*Tænia crassicollis.*) and the Cysticercus which is found in the liver of the rat and mouse (*Cysticercus fasciolaris*). C. T. von Siebold, the most noted helminthologist now living, had observed the same thing, and in 1848 had already alluded to the possibility that all these Cystica might be nothing but undeveloped or larval tapeworms. In his system, however, he still recognized the *Cystica* as a distinct order of Helminths.

In the year 1851, F. Küchenmeister first proved by experiment that a certain hydatid when brought into a suitable place, is developed into a tapeworm. He fed a dog with the hydatids (*Cysticercus pisiformis*) found in the mesentery of the hare, and on dissecting the dog, after a number of weeks, *found these Cysticerci alive in the small intestine. They had, however, lost their tail-bladder, and the neck had begun to form the joints of a true tapeworm*, which worm had been long well known as *Tænia serrata*, and as common in the dog. Now, one discovery followed another. Governments, scientific institutions, and wealthy farmers furnished the money and animals to carry on the experiments on a large scale. Siebold fed a dog with the *Echinococcus* of the ox, and thus raised the *Tænia Echinococcus*, Siebold. It was also found in the same way that the *Cœnurus* from the brain of sheep is the larve of another *Tænia* of the dog, *Tænia Cœnurus*, Siebold.

Now the question, whence does man get his tapeworm? was ready to be answered. It had been observed that the hydatids of the hog, commonly called "measles" (in the zoological system, *Cysticercus Cellulosæ*,) have exactly the same head as the common tapeworm of man (*Tænia Solium*, L.); and after the experiments mentioned above, in relation to the different tapeworms of dogs, a doubt could hardly exist *that Cysticercus Cellulosæ of the hog was the larve of the common human tapeworm* (*Tænia Solium*). Küchenmeister, who wished to make sure of the fact, made the experiment upon a criminal who was soon to be executed, and, as was to be expected, with perfect success. Measles taken from fresh pork, and put into sausages which the criminal ate raw, at certain intervals before his death, were found again, in the post-mortem examination, as tapeworms in his intestine, and in different stages of development, according to the intervals in which the measles had been taken.

Thus it became clear, that all hydatids are tapeworm larves, which, when swallowed with the animal, or a portion of it, in which they live, by another animal, develop in the intestine of the latter. * * * *

Now the opportunity for experiments was again open in another direction. If the tapeworm embryo developed its scolex or head by interior budding, it was likely that those animals having hydatids got them by eating the eggs of the species of tapeworm to which those hydatids belonged. And this has been proved by experiment. Goats fed with eggs of the *Tænia Echinococcus* got the Echinococcus; sheep fed with the eggs of *Tænia Cœnurus*, got the Cœnurus in their brain; *healthy young hogs fed with the eggs of the human tapeworm got the measles.* Küchenmeister, Siebold, Van Beneden, Gurlt, Luschka, Wagener, Leuckart, Eschricht, and others, have the merit of tracing this interesting development. From their further investigation, it became moreover evident, that the Cœnurus also, with its many heads, originated from *one* embryo, which, enlarging greatly, throws out as buds from its interior, not one, but many scolices; moreover, that the process is also exactly the same in *Echinococcus*, except that in this hydatid the scolices free themselves after a while from the internal walls of the bladder, and thus swim in the fluid contained in the bladder, the latter itself being simply the enlarged embryo.

But the zeal of these investigators did not rest here. If the sheep gets by chance the eggs of the *Tænia Cœnurus* of the dog into its stom-

ach, how do the embryos hatching from those eggs reach a suitable place for their development into hydatids, which place is, in the sheep, the brain? It had been erroneously assumed that they bored with their spines *recta via* from the stomach through all the tissues and organs until they reached the brain. Accordingly, in the hog, the embryos of the Tænia would have to go from the stomach into the muscles; in the rat, into the liver; and in the ox, into the lungs; for it is only in these particular organs that these hydatids are found.

R. Leuckart, however, discovered the way in which the embryos actually reach their destined resting places. On feeding rabbits with the eggs of *Tænia serrata*, he found that, some hours after the feeding, the egg-shells were already dissolved into prismatic granules by the juices of the *stomach*, and the embryos set free. But on putting the eggs immediately in the *intestine* (through an artifical opening,) they were not hatched. It was clear, therefore, that only the gastric juice could hatch the embryos; and this accounts at once for the strange fact, that the embryo never hatches in the intestine of the animal where the tapeworm itself lives. Moreover, he found that they do not pass from the stomach into the intestine, and hence, as had been supposed, through the bile-ducts into the liver, but that they pierce the blood-vessels, and *thus come into the circulation.* He even, after a long search, found four perfect embryos in the blood taken from the *vena portæ. It is by the blood that the embryos of tapeworms are carried to the organs in which they develop into hydatids.* It now at once became obvious how easily they reach the muscles, the brain, the lungs, etc. But it is to be supposed that only those which reach the destined organ will develop themseves, while the rest, which are carried to other organs, must perish."

The subject is continued with a full description of the common tapeworm and of other species. The extreme length is stated by Diesing at twenty-four feet.

4. *Depth of Molluscs of Peconic and Gardiner's Bays, Long Island, N. Y.;* by SANDERSON SMITH. (Communicated for this Journal.)

Name.	Depth.	Remarks.
Loligo illecebrosa,		Large and abundant.
Ranella caudata,	— to 10 f.	Moderately abundant.
Pyrula canaliculata,		Abundant.
Pyrula carica,	L. w. to 10 f.	"
*Buccinum plicosum,	H. w. to 10 f.	Large and abundant.
*Nassa obsoleta,	Littoral.	Very abundant.
*Nassa trivittata,	2 f. to 10 f.	Abundant.
*Columbella avara,	L. w. to 10 f.	Moderately abundant.
*Columbella Gouldiana,		Rare.
Columbella lunata,	L. w. to 10 f.	Mod. abundant.
Pleurotoma cerinum,*		Rare.
Pleurotoma plicatum,	2 f.	Not so rare as preceding.
*Natica heros,	10 f.	Rare and small.
*Natica duplicata,	10 f.	" " "
*Natica triseriata,	2 f. to 10 f.	Mod. abundant.
Natica pusilla,*		One dead specimen.

Names.	Depth.	Remarks.
*Natica immaculata,		One dead specimen.
Eulima subangulata,*		Rare.
Chemnitzia producta,*		"
*Chemnitzia fusca,		"
*Chemnitzia seminuda,	2 f.	Only once found, numerous.
Chemnitzia trifida,	Low water.	Moderately abundant.
Chemnitzia bisuturalis,	Low water.	Rare.
*Chemnitzia interrupta,	4 or 5 f.	"
Scalaria clathrus,*		One dead specimen.
Scalaria lineata,*		" " "
Cerithium Sayi,	L. w. to 2 f.	Extremely abundant.
Cerithium nigrocinctum,*	L. w. to 10 f.	Rare.
Cerithium Greenii,	L. w. to 2 f.	"
Cerithiopsis Emersonii,*	4 f. to 10 f.	Moderately abundant.
Cerithiopsis terebellum,*	4 f. to 10 f.	" "
Cæcum pulchellum ?*	In sand at 10 f.	Abundant.
Vermetus radicula,*		The tip of one specimen.
*Littorina rudis,	Littoral.	Very abundant.
*Littorina littoralis (animal white),	Littoral.	Abundant.
Littorina " (animal black),	Littoral.	"
*Lacuna vincta,	Low water.	Moderately abundant.
" " var. fusca,	" "	" "
*Rissoa minuta,	Low water.	Extremely abundant.
Skenea ? n. s.*		One dead specimen.
*Calyptrea striata,		" " "
*Crepidula fornicata,	H. w. to 10 f.	Very abundant.
*Crepidula convexa,	L. w. to 10 f.	Abundant.
*Crepidula unguiformis,	L. w. to 10 f.	"
*Tectura testudinalis,	Low water.	Moderately abundant.
Chiton apiculatus,	4 f. to 10 f.	" "
*Melampus corneus,	Littoral.	Very abundant.
Actæon punctostriatus,		Rare.
*Bulla solitaria,		"
*Bulla canaliculata,	2 f.	Not so rare as preceding.
Æolis, n. s. ?	Low water.	One specimen.
Ostrea borealis,		Rare.
*Anomia ephippium,	H. w. to 10 f.	Very abundant and large.
" vars. electrica and squamula.		
Anomia aculeata,		Rare.
*Pecten irradians,	L. w. to 3 or 4 f.	Extremely abundant.
*Mytilus edulis,	Littoral.	Not very abundant.
*Mytilus modiolus,	— to 10 f.	Abundant.
*Mytilus plicatulus,	Littoral.	"
Arca transversa,	3 f. to 10 f.	"
Arca pexata,		Rare.
Nucula proxima,	2 f. to 10 f.	Abundant.
Leda limatula,	2 f. to 3 f.	Rare.
Leda sapotilla,	3 f.	"
*Solemya velum,	4 f. to 10 f.	"

Names.	Depth.	Remarks.
Solemya borealis,		A fragment.
Cardium Mortoni,	l. w. to 1 f.	Abundant and large.
*Cardium pinnulatum?		One valve.
Astarte mactracea,*		Dead specimens abundant.
*Venus mercenaria,		Moderately abundant.
*Cytherea convexa,		One valve.
*Venus gemma,	— to 2 f.	Very abundant.
Petricola dactylus,		Rare.
*Petricola pholadiformis,		"
*Mactra lateralis,	2 f. to 6 f.	"
*Mactra solidissima,	10 f.	Rare and small.
*Kellia planulata,		"
Montacuta elevata,		One valve.
*Tellina agilis (St.),	2 f. to 6 f.	Moderately abundant.
Tellina tenta,*	— to 6 f.	" "
*Tellina fusca,	— to 6 f.	" "
Cumingia tellinoides,*		" "
*Solen ensis,		Not very abundant.
Solecurtus bidens,*		Rare.
*Mya arenaria,	Littoral.	Very abundant.
Corbula contracta,		Abundant.
*Anatina papyracea,	3 f.	Two specimens.
*Cochlodesma leanum,	3 f.	Rare.
*Lyonsia hyalina,	— to 6 f.	Moderately abundant.
Thracia Conradi,		A few odd valves.
*Pandora trilineata,	3 f. to 6 f.	Rare.
*Saxicava distorta,		"

Turdo, Theca, Ascidia, a Cynthia, a Molgula, two or three Aplysiæ, and six or seven Botrylli and Polyclina.

Recapitulation.—One Cephalopod, forty-three Prosibranchs, one Pulmonifer, three Tectibranchs, one Nudibranch, forty-one Lamellibranchs, and fifteen Tunicates, altogether one hundred and five marine species. Besides these, *Astarte castanea, *Cyprina islandica,* *Mesodesma arctatum, *Purpura lapillus, and *Buccinum undatum, occur on the Sound and about Montauk Point, making a total of one hundred and ten species for the eastern end of Long Island. Twenty-nine of these (marked with a * after them) excluding the Tunicata, are additional to those stated by Dekay to occur in the waters of the State, though many of them are surmised by him to exist there. I have no access to a library, to determine how many have since been described as coming from them. Including the Tunicata, the number would rise to forty-three or forty-four. Sixty-two species (marked with a * before them), or sixty-five per cent, (excluding the Tunicata,) pass Cape Cod. Only twenty-nine other species are stated by Mr. Stimpson, in his "Shells of New England," to pass the Cape, so that 68·1 per cent of the whole number occur here; and a little dredging about Montauk would probably discover nearly all the others.

IV. ASTRONOMY.

1. *Fifty-fourth and fifty-fifth Asteroids.*—The asteroid discovered Sept. 10, 1858, by M. Goldschmidt at Paris, has been named *Alexandra*, and is numbered as the *fifty-fourth* of the series. The asteroid discovered on the same night, by Mr. George Searle at Albany, N. Y., has been named *Pandora*, and is numbered the *fifty-fifth*.

2. *Another Asteroid.*—In 1857, Mr. E. Schubert of Washington, undertook a series of observations of the asteroid *Daphne*. On computing his observations he was surprised to discover that he had not found Daphne but had observed for it a new asteroid in the neighborhood. He has computed its elements, and it is to be hoped that the body will be redetected.

3. *Review of Gilliss's Astronomical Observations in Chili,** (from Gould's Astronomical Journal, 1858, p. 168).—This volume, though bearing the date of the year in which the observations were printed, has only been issued a few weeks. It contains 332 pages of observations of *Mars* and *Venus* during two oppositions of the former and inferior conjunctions of the latter, made at Santiago by Lieut. Gilliss or under his superintendence. These series comprise both micrometric comparisons with the equatorial, and absolute determinations with the meridian circle. These are followed by 69 pages from the Washington Observatory, containing a description of the equatorial and a series of micrometric observations by Mr. Ferguson of each of the four oppositions or conjunctions. A portion contributed by Mr. Bond of the Cambridge Observatory contains 43 pages of observations of *Mars* during the opposition of 1849–50, chiefly micrometric determinations of right-ascension. Finally, Mr. Maclear, of the Royal Observatory at the Cape of Good Hope, has furnished an extensive series of micrometer-comparisons with the preselected stars, during the first opposition of *Mars*.

These 492 pages of observations and accompanying remarks are preceded by introductory remarks upon the origin and operations of the expedition, with a description of the instruments and method of observation employed, by Lieut. Gilliss; and by a detailed discussion of the entire mass of observations by the editor of this Journal. This discussion occupies 264 pages.

The plan of the expedition contemplated micrometric comparisons of the limbs of the planets, with stars previously selected by Lieut. Gilliss for the purpose, simultaneously made in the northern and southern hemispheres; but the extremely small number of northern observations precluded all hope of attaining any valuable addition to our knowledge of the Solar Parallax by this method.

In the earnest desire that so extended and costly a series of careful observations should not prove futile for the attainment of the desired end, a method of discussion has been employed, which, though entailing an inordinate amount of toil, seemed to afford the only adequate means of rendering the observations serviceable for the fulfilment of their design. The method may be briefly described.

* The U. S. Astronomical Expedition to the Southern Hemisphere, during the years 1849–52. Vol. III: Observations to determine the Solar Parallax, by Lieut. J. M. Gilliss, LL.D., Superintendent. Washington, 1856. 4to.

A catalogue of the comparison-stars having been prepared, their declinations were obtained from a thorough examination of all the available sources, and from the combination by weights of the positions as given by the several authorities. Regard being had to the existence of any possible proper motion, the positions obtained were referred to the mean equinox of the beginning of the year in which the comparisons were made, and a final list of comparison-stars thus constructed, containing not only the declinations of each star, but the relative value of the determination.

The complete reduction of all the observations was then repeated, and the several comparisons with each star consolidated with care into a single observation, of which the weight was determined, and which was subsequently treated like an absolute determination of place, and even combined with meridian observations. Each of the four series (viz. the two *Mars*-oppositions and the *Venus*-conjunctions) was treated independently, and the error of the ephemeris considered as of the form $x + \tau y + \tau^2 z$, τ denoting the time from a medial epoch. Four other unknown quantities were introduced,—two of these relating to the apparent semidiameter, one depending on the micrometer employed, and the last being the correction to Encke's determination of the mean solar parallax. The observations at each place have been independently discussed, and the several groups finally combined in series of approximate solutions by least squares.

The sequel indicates that the results afforded by the series of observations of *Mars*, during the opposition of 1849–50, so far surpass any of the others in precision and trustworthiness, that these, taken alone, promise a closer approach to the desired values than when combined with the three other series.

The resultant determinations of the whole discussion are as follows:

Semidiameter of *Mars*	$= 4''\cdot6639 + 1''\cdot9681 = 6''\cdot63$
" " *Venus*	$= 8\cdot6625 - 0\cdot3118 = 8\cdot35$
Mean Solar Parallax	$= 8\cdot5712 - 0\cdot0762 = 8\cdot4950$

The great increase of the previously assumed semidiameter of *Mars* is very striking. It has proved impracticable in most cases to free the semidiameter from the possible influence of an irradiation dependent on the observer or on the telescope. The adopted value depends on the Washington and Santiago observations only. The necessity of some increase to the previously adopted value is indicated by six series of observations; being all made, during the first opposition, with the exception of the Cape of Good Hope series.

The propriety of a small diminution of the adopted diameter of *Venus* seems also to be distinctly and strongly pointed out.

Upon the resultant value of the parallax I am not inclined to place any great stress, but cannot refrain from expressing the decided conviction that the value obtained by Encke from the transits of *Venus* in 1761 and 1769 may be improved by a slight decrease; and am inclined to regard the value $8''\cdot5000$ as being in all probability quite near the truth, and that this value may be advantageously adopted. G.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Mountains of North Carolina and Tennessee;* by S. B. BUCKLEY. (Communicated for this Journal).—During the summer of 1835, Professor Elisha Mitchell of Chapel Hill University, North Carolina, measured the highest point of the Black Mountain in that State, and announced its height to be 6476 feet. His stationary barometer was at Morganton, which he *estimated* to be 968 feet above the sea. The late railroad surveys show that the Morganton depot is 1169 feet high; making the place where Dr. Mitchell's stationary barometer hung, 1200 feet above the level of the sea. Hence the height of Mount Mitchell—the name which has justly been given to the highest part of the Black Mountain—according to the indications given by Prof. Mitchell's barometer in 1835, is 6708 feet. This measurement of 1835 was first published in the Raleigh Register, and again in Silliman's Journal in 1839, with some additional remarks by Dr. M., in which he alludes to the great apparent height of the mountains in Haywood county, and also to the highest in the Great Smoky Range. The Highland Messenger, published at Asheville, near the Black Mountain, in 1840 when alluding to Dr. M.'s measurement of it, says: "we are perfectly willing to concede the name of Mount Mitchell to that particular point of the Black Mountain which Prof. Mitchell, after a degree of labor and expense, which none other than a genuine devotee of science would have incurred, demonstrated to be the most elevated point of *measured* land east of the Rocky Mountains. We say measured land, because we have long believed, and still believe that there is one, if not two points, in the same range of mountains higher than that measured by Prof. Mitchell, from forty to sixty miles west of the Black Mountains." This is from an editorial by the Rev. D. R. McKally, D.D., now editor of the Christian Advocate at St. Louis, Missouri. We have quoted it, because his *higher points* are probably the two highest which we have recently measured in the Smoky Range, about sixty miles nearly west of the Black Mountains.

In the Transactions of the Smithsonian Institution for 1855, is Mr. Clingman's account of the Black Mountain, the highest point of which he estimates to be 6941 feet, which is 233 feet higher than Prof. Mitchell's corrected height of the same point. Prof. Turner the engineer has since found its height to be 6711 feet, and in 1856 Prof. Guyot by a series of barometrical observations, ascertained it to be 6701 feet high. There is little discrepancy between the measurements of Professors Mitchell, Turner and Guyot, and hence there can be little doubt that Mr. Clingman's estimated height of the Black Mountain, as first given in the Smithsonian Transactions, and now in Colton's new atlas of the World, and also in Lippincott's Gazetteer, is at least 230 feet above its true height.

Prof. Mitchell in 1838 and 1844 again visited the Carolina mountains, at which time his stationary barometer was at Asheville. The following measurements, then made, are taken from a letter of his, published in an Asheville newspaper.

	Above the sea.
"Asheville,	2200 feet.
French Broad river at Asheville,	1977 "
Lower Ford of Pigeon,	2475 "
Waynesville,	2722 "
Head of Scott's Creek,	3240 "
Tuckaseige Ford,	1927 "
Gully Whee Gap,	3897 "
Blue Ridge head of Tuckaseige,	3795 "
Col. Zachary's Cashiers valley,	3324 "
Chimney Top,	4433 feet.
" " above Zachary's,	1109 "
Burnsville,	2763 "
Top of Black Mountain,	6772 "
Morganton,	1081 "
Table Rock,	3584 "
Grandfather,	5719 "
Roane,	6187 "

It should be remembered that these measurements were also made previous to the railroad surveys, by which it is now known that the height of Asheville near the court house is 2260 feet.

For the convenience of future observers we give below, Prof. Guyot's measurements in 1856, in and around the Black Mountains, the three last excepted.

	Above the sea.
Jesse Stepps, Lower Mountain house, Swaninoa valley,	2770 feet.
Terminus of carriage road up Black Mt. to Wm. Patton's Mt. House,	3244 "
Potatoe Top,	6389 "
Mitchell's Peak,	6577 "
Mount Gibbes,	6586 "
" Haulback,	6401 "
Sandoz Peak,	6612 "
Cattail Peak,	6595 "
Rocky Trail Peak,	6486 "
Deer Mountain,	6216 "
Long Ridge Middle Peak,	6253 "
Bowlen's Pyramid at north end of the Black,	6345 feet.
Wm. Patten's Mt. House,	5248 "
Mt. Mitchell, "highest,"	6701 "
Guyot's Peak,	6661 "
Hairy Bear,	6597 "
Junction of Cattail Fork and Caney river,	2824 "
Burnsville court house square, near Penland's Hotel,	2819 "
Mount Pisgah,	5760 "
Roane Mt.,	6318 "
Grandfather, measured in 1858,	5897 "

Prof. Guyot remarks in a letter to us containing his measurements in 1856, that "these heights may be modified by a few feet in" his "final publication, the point of base not being identified within three feet."

The following are the heights of some mountains and places in North Carolina and Tennessee, south and west of Asheville, which were measured by us with two of Green's standard barometers during the months of September and October in 1858. Prof. J. LeConte of Columbia, S. C., observed the stationary barometer at Waynesville, N. C., for the measurement of most of the highest Smoky Mountains, but being called away by the duties of his professorship, the stationary barometer was removed to Col. Cathey's, at the Forks of Pigeon, Haywood Co., N. C., and placed in charge of Miss S. Cathey. We also received material assistance from Mr. T. J. Lenoir and Mr. Turner Cathey, during our mountain excursions.

	Above the sea.
Waynesville,	2815 feet.
Col. Cathey's,	2750 "
Platt's Peak,	6196 "
Jones' "	6337 "
Amos Platt's Balsam,	6406 "
Cold Mountain,	6105 "
Shining Rock,	6063 "
Father Old-Field,	6116 "
Hyman's Peak,	6095 "
Cathey's "	6240 "
Wilson's Balsam,	6270 "
Mount Hargrove,	6156 "
Devil's Court House,	6057 "
Lenoir's Bald Mt.,	6040 feet.
Mount Hardy,	6257 "
Mount Lenoir,	6413 "
N. Peak of Mt. Lenoir,	6399 "
Sarah's Mountain,	5993 "
Mount Cathey,	5742 "
" Starling,	6456 "
" Emmons,	6465 "
Flat Creek Balsam,	6087 "
Whiteside,	5076 "
Top of Whiteside to base of precipice	1510 "
Mount McDowell,	5100 "

The following points are in the Smoky Mountains, and many of them are on the State line, between North Carolina and Tennessee.

E. P. Hopkins's house,..... 1995 feet. | White Rock Mountain,..... 5002 feet.

This last is a misnomer of the hunters, being composed of a dark gneiss and mica slate, covered in many places with white lichens, the most abundant of which are Cladonia rangiferina, and Cladonia Caroliniana.

Mount Safford, ...	6296 feet.	Old Field Knob,.....	6220 feet.
" Henry, ...	6425 "	Peck's Peak,.....	6338 "
" Guyot,.....	6734 "	Safford's Peak,.....	6559 "
" Floyd, ...	6073 "	Mount LeConte,.....	6670 "
" Mingus,.....	5779 "	Mount Buckley,.....	6755 "
Summit of Road Gap near the Alum Cave,.....	5314 "	Curtis' Peak,.....	6511 "
		Mount Collins,.....	6241 "
Right Hand Gap,.....	5162 "	Robert Collins House,.....	2535 "
Mount Ocona,.....	5978 "		

It is proper to state that most of these heights are the result of a single barometrical observation, and hence they will probably be modified somewhat by future observers. Observations were made on the two highest at two different visits, and a mean result between the two calculations is given as the height of Mount Buckley, while the height of Mount Guyot is given as ascertained by the first visit, it being made in a more settled state of the weather. The second observation at its summit gave its height as 6994 feet. It is well known to those conversant with the barometrical measurement of heights, that accuracy requires a series of observations, and it was out of our power to make them at so many points during the time to which we were limited by the lateness of the season.

Fortunately the months of September and October were uncommonly dry, which enabled us to continue exploring nearly the entire time. The toil was great, and the difficulties to be encountered can only be imagined by those who have ascended the steeps of the unfrequented Southern Alleghanies, through laurel thickets (Rhododendrons and Kalmia,) and multitudes of the prickly locust, (Robinia hispida,) which has a *penchant* for scratching the face and hands, tearing the clothes, and occasionally the skin beneath. We found the Viburnum lantanoides or hobble-bush with its straggling branches, very troublesome on the Smoky Mountains. Notwithstanding all this we have the mountains and their glorious scenery. We encamped eleven nights on their tops; and saw that the stars were brighter, and the planets apparently larger than when seen from the valleys below. Then also the wonderful comet (Donati's) made the southwest luminous with its bright head and mysterious tail, soon after the setting sun.

The scenery of these mountains, especially those in the Smoky Range, abounds in precipices and deep chasms, surpassing any thing we remember to have seen among the White Mountains of New Hampshire. The spectator on the highest Smoky Peaks can enjoy a more varied view than from any other points in the Southern Alleghanies. East Tennessee with its towns, rivers, and the Cumberland mountains in the distance, is spread beneath at the west. On the north can be seen the Clinch mountains extending into Kentucky. At the northeast, east, and southeast, in full

view are all the higher mountains of North Carolina, and at the south the smaller ones of Northern Georgia. Such prospects *pay* the explorer for his toil; their remembrance is always sweet. The country on the Tennessee side is much lower than in South Carolina, and the descent of the Smoky mountains is generally more abrupt and precipitous into the former State, than into the latter.

The highest Smoky mountains are near the head waters of the Ocona-luftu and Little Pigeon rivers, being accessible from Tennessee via Sevierville, and up the Little Pigeon to a Mr. Hawkins', who lives eight miles from the top of the gap road, which is near the alum cave; and from North Carolina by the road up the Ocona-luftu to Mr. Collins's house, seven miles from the top of the afore-named gap-road.

The geology of the mountains south and west of Asheville has a good deal of sameness, they being composed of crystalline rocks, with the exception of a narrow strip, extending southwest along the Unaka or Smoky mountains which belongs to the taconic system of Emmons. The taconic rocks here consist of dark colored shales in which we do not remember to have seen any organic remains. The strata of these rocks are in many places nearly and often quite vertical. They are well exposed along the Middle or Straight Fork of the Ravensfork in descending from Mount Guyot to the Ocona-luftu. They also occur at the summit of the gap-road near Mount Mingus, and extend two or three miles down the road into North Carolina. The chief rocks of the Haywood mountains are granite, gneiss and mica slate, excepting a small portion near the Smoky Range, where the taconic rocks are again found. The Shining-Rock mountain about eleven miles south of the Forks of the Pigeon is entirely of white or milky quartz, and is probably the largest mass of that rock at any one point in the Alleghanies. It has a fine appearance in the distance and is deservedly becoming quite a place of resort. We believe that Haywood and Jackson counties, N. C., have not as yet afforded any paying mines to those who have been at the expense of working them, but it must be admitted that they have been little explored for that purpose. Prof. Emmons the State Geologist, contemplates a survey of those mountains next summer, and we suspect that he will destroy the golden dreams of a few who build castles upon undeveloped mineral wealth.

This region has long been a favorite place of resort for the botanist. Here there is a strange mixture of northern and southern species of plants, while there are quite a number which have been found in no other section of the world. In the months of May and June when the Kalmia, Rhododendrons and Azaleas are in bloom, these mountains and valleys present an array of floral beauty which is indigenous to no other section of the United States. The much vaunted western prairies with their interminable sameness, are by no means as beautiful. The Rhododendron Catawbiense, Kalmia latifolia and Azalea calendulacea, are not excelled by any native floral beauties; the two last abound in nearly every section of these mountains, but the first rarely descends into the valleys. Besides these the Rhododendron maximum, (laurel,) Rhododendron punctatum, Azalea arborescens and nudiflora, Oxydendrum arboreum, Chionanthus Virginica, Halesia tetraptera, Clethra acuminata, Robinia hispida and

viscosa, Stuartia pentagyna, Liriodendron tulipifera, Magnolia acuminata, Umbrella, and Fraseri, grow there more or less abundantly, and they are all ranked as among the most ornamental trees and shrubs of the Atlantic States. The Pyrus Coronasia is very common south of the French Broad river; Catalpa occurs in several places along the same river and in the mountain valleys near the Warm Springs; Cladastris, grows at Paint Rock, Tenn., which is near the Warm Springs. Most of the highest mountain tops are covered with the Abies nigra and Abies Fraseri: the former is the black spruce, and is erroneously called the balsam; the latter is the true balsam with blisters in its bark, from which balsam is collected. It attains a greater size than Pursh or Nuttall have given it in their works. We measured some on Wilson's Balsam and near Cathey's Peak, which were more than three feet in diameter and from eighty to one hundred feet high. The black spruce appears to grow at a lower elevation than the balsam, but neither of them are often met beneath an height of 4000 feet.

The banks of streams and coves of these mountains have some of the largest trees in the United States east of Mississippi river. There is a Tulip tree or Poplar (Liriodendron tulipifera,) near the Pigeon river in Haywood Co., N. C., about eight miles from the Tennessee line, thirty-three (33) feet in circumference at three feet from the ground, or eleven feet in diameter, and upwards of one hundred feet high. Another on the western slope of the Smoky mountains in Tennessee, on the Little Pigeon river, is twenty-nine feet in circumference at three feet from the ground. Near this locality we also measured a chestnut (Castanea vesca,) thirty-three feet in circumference at four feet from the ground. It is a noble living specimen, apparently sound, and of nearly a uniform diameter upwards, for forty or fifty feet. About two miles farther up the same stream there is a hemlock, or spruce pine, (Abies Canadensis) nineteen feet and two inches in circumference at four feet from its base. Here also the Halesia tetraptera attains an uncommon size, being from two to three feet in diameter, and about sixty feet high. On Jonathan's Creek there is a white oak (Quercus alba,) nineteen feet in circumference at three feet from the ground. This list of large trees could greatly be extended, but enough have already been cited to show the richness of those coves and valleys.

The Quercus Leana of Nuttall occurs at several places on the Tennessee river near Franklin in Macon Co., North Carolina. It is evidently there a hybrid between Quercus imbricaria and Q. tinctoria. Its acorns are identical with those of the Q. imbricaria. On the Haywood mountains we saw a few specimens of the Betula excelsa (yellow birch), and Mr. Curtis says he found it on the Black mountain. Among several shrubs which we obtained for cultivation the Pyrularia oleifera or oil-nut is peculiarly interesting. It grows to the height of from five to ten feet, and bears a pear-shaped fruit little more than an inch in diameter, which is so oily that it will burn like a candle if a wick be drawn through it. Squirrels are fond of it, and cattle have a great liking for the young branches and leaves of the Pyrularia. Last spring we saw an abundance of it in the edge of some woods fenced into a wheat field, and in October we again went there after the fruit; but the harvest was past, the

field had been pastured with cattle, which had destroyed nearly all of the Pyrularia. Hence it has already become rare, and the general occupancy of the mountains with herds of cattle and flocks of sheep would soon destroy it entirely. Mr. Durand of Philadelphia thinks that the oil expressed from it is superior to the best olive oil. Our specimens of the Pyrularia have been planted at Philadelphia, New York, and at the botanic garden of Cambridge, near Boston, and also some of them have been sent to Paris to the Acclimating Society of France, whose object is to acclimate useful trees, shrubs and plants.

On Mount Mingus we first met with the Rugelia, a new genus of Shuttleworth, in the natural order Compositæ, which has not yet been described in American works on botany. It is frequently found along the Smoky mountains to the extent of twenty-five or thirty miles. Dr. Gray recognized it at once, he having received it from Mr. Shuttleworth, a European botanist to whom Rugel sent plants. Sixteen years before, in the early spring, we had visited those same mountains with Dr. Rugel, a German botanist, and we were right glad to learn that his name was affixed to one of their interesting plants. The Solidago glomerata grows on most of the Balsam mountains, and the Potentilla tridentata of the New England mountains also grows on the bald peaks of Macon county, North Carolina.

The Carolina mountains have a great variety of huckleberries (Vaccinium and Gaylussacia) ripening in succession from July to September. When we first met with acres of those bushes, in September, covered with large delicious fruit, the temptation was so great that we partook rather freely, expecting to pay the penalty of over indulgence, but were happily disappointed. Judging from the experience of others and our own on many occasions, those berries are remarkably healthy. Most of them were larger than any we ever saw at the south. The Vaccinium Constablei of Gray, which sometimes grows ten or fifteen feet high (on Shining Rock), was covered with ripe fruit as late as the middle of October. There are several species of the huckleberry which are worthy of cultivation. The common high blackberry (Rubus villosus) is often found in dense patches on and near the mountain tops, with its stems smooth, and destitute of prickles. This rule is constant. We do not remember to have met with an exception. The same species growing in the valleys has its stems armed with prickles.

In the month of September many of the women and children dig "sang," (Aralia quinquefolia,) in the valleys and on the mountain sides. The dry roots of the ginseng or "sang," as it is always there called, are worth at home twenty-five cents per pound. We met with one man who had bought 30,000 pounds, and we remember being with one family whose children sold seventy pounds of dried sang. These roots are dug with a long narrow hoe called the "sang hoe."

Snow birds (Fringilla nivalis) we saw on the Black mountain, and also on many of the other Balsam mountains south and west of Asheville. They were solitary or in pairs, showing evidently that they breed in those places. Another species of bird, whose summer habitat is generally supposed to be confined to the north, also breeds and summers in those Balsam mountains. It is the Crossbill (Loxia curvirostris) whose curious

bill is well adapted to extract seeds from the cones of the black spruce and balsam trees. In the mountain valleys we frequently met with many northern birds, among which was that sweet songster, the rose-breasted Grosbeak (Fringilla Ludoviciana).

The tedium of the night, when encamping on the mountains, is almost always enlivened by the stories of the guides and their adventures in hunting. They all positively assert that the bears in early spring, when first emerging from their winter quarters, are as fat as when they first retire for the winter. During the winter they shed the soles of their feet, which renders their walking difficult in the first of spring, when their food consists of the young plants, on which diet they soon become lean, and remain so until the ripening of berries in August and September. They are very fond of hogs and pigs, pork and honey being their favorite diet. Why they bite and scratch the bark and limbs of the balsam and black spruce we cannot tell. It cannot be for food, because they do not generally leave the marks of their teeth on a tree, except in one or two places. Sometime they rise on their hind legs and make long deep scratches in the bark with their fore paws. It may be done for sport, or to let their companions know their whereabouts. We have seen those fresh bites and scratches on different trees at all seasons of the year. The bears show great sagacity in feeding at the leeward of the paths on the mountain ridges, along which the hunter is almost obliged to travel, hence if the wind blows it is almost impossible to get a shot at them, their keen scent discovering the hunter long before he gets within shooting distance. They are stupid and unwary about traps, entering without fear the log pens; these are shallow, with a depth of not more than two feet, over which is raised a very heavy top, which falls and crushes the bear when he disturbs the bait. Hundreds are caught in this manner every year. In the unfrequented parts of the mountains the large steel trap is concealed in the bear trail; but this is dangerous, and liable to catch dogs, of which we saw two caught in one morning to our great sorrow. The piteous yells of those unfortunate dogs rang in our ears long afterwards. The bears rarely disturb calves or young cattle, but in one locality of the Smoky mountains we were told that they did much damage in killing young cattle, and that there could be no mistake about it, because a large bear had been caught in the act of killing a young steer. The panther, wild cat, and wolf are all troublesome to the mountain farmer of those regions. The panther destroys sheep and hogs; the wild cat, lambs and pigs. Both are cowardly and thievish, being rarely seen.

The Red squirrel (Sciurus Hudsonius) called Mountain Buman in North Carolina, is common on all the higher mountains. They rarely descend into the valleys. They are fond of the seeds of the balsam and black spruce, and as they are rarely molested by the hunters, they are very noisy, active, and more fearless of man than their brothers at the north. The Ground squirrels (Sciurus striatus) are also very abundant, often destroying a good deal of corn, but as corn is plenty, and larger game common, the ground squirrel is rarely killed. We were told by a travelling fur merchant, whom we there met, that the skins which he bought among the mountains, equal in fineness and goodness those of

the north, and that northern merchants could not tell the difference; still in order to get the highest price he was obliged to send his skins to New York, through Ohio and *via* the Erie Railroad as if they had come from the northwest. The principal furs obtained in the southern Alleghanies are the skins of the otter, mink, black fox, red fox, raccoon, and muskrat.

From the great height of the southern Alleghanies, there being twenty-four peaks higher than Mount Washington, it will be readily inferred that they have a northern climate. A year ago, our guide to the top of Roane told us that he had been on its summit when it was covered with snow on the 17th of June. There is a table land extending from near the Roane to the head of Turkey Cove and Linville Falls, a distance of twenty or thirty-five miles, on which the inhabitants succeed with difficulty in raising Indian corn sufficient for their own consumption. Occasionally thay have frost during every month in the year, and then they resort on horseback or on foot to the valleys for corn. About the first of last May we saw the mountains in Haywood covered with snow about six inches deep. The wheat harvest at the Forks of Pigeon begins about the first week in July; and we know of no better criterion for isothermal lines than the time of ripening wheat. We kept a record of it in western New York, and in ten years the annual time of beginning the wheat harvest did not vary three days from the 16th of July.

The valleys in the Carolina Mountains vary in elevation from two thousand to upwards of three thousand feet, hence a few miles travel will often take one to a much warmer or colder climate. This we experienced very sensibly in going from the valley of Jonathan's Creek to that of the Soco River. The former has a mean elevation of about three thousand feet and the latter near two thousand. The Chinese sugar-cane (Sorghum) is extensively grown, and may be regarded as a decided success. There are few portions of the Union where such a production is more needed. The absence of railroads and the cost of transportation render sugar and molasses dear; hence the introduction of the Chinese sugar-cane in that section is a great blessing, and will enable many a poor family to have sweet coffee.

In no section of the United States have we seen finer apples, and they are mostly from seedlings originally planted by the Indians. Silas McDowell of Franklin, in Macon Co., has devoted more than twenty years to the selection and grafting of those best native apples, and he now has an orchard of more than 600 apple trees, which bear fruit equal if not superior to the best northern kinds. There is said to be a line or belt on the mountain sides about three hundred feet above the adjoining plain or valley, and extending upwards several hundred feet, where fruit trees always bear, because the belt is free from frost. If this be true,—and we believe its truth has been pretty well tested by experiment,—the mountains of North Carolina might supply the South with an abundance of the choicest fruit, if the means of transportation were good. By the cultivation of more grass, and the introduction of the improved breeds of cattle into those mountain valleys, butter and cheese might also be made for the southern market. One great drawback to the raising of sheep is that they are destroyed by wild animals, and also killed by the

dogs. Still we think it would even pay well to keep sheep, herd them at night, and have a shepherd with his dog to guard them by day, and thus revive old Arcadian times among those delightful mountains.

2. *On some Modified Results attending the Decomposition of Bituminous Coals by Heat;* by Dr. A. A. Hayes.—When bituminous coal is exposed in proper vessels to a gradually increasing temperature, at a certain point decomposition commences and continues, while heavy hydrocarbon vapors, mixed with the vapors of water and salts of ammonia, escape, and may be condensed.

The proportion of permanent gases formed is small in comparison with the weight of the liquids produced, when the decomposition of the coal is carefully regulated.

In the ordinary rapid breaking up of the composition of coal by heat suddenly applied in the manufacture of illuminating gas, the proportion of permanent gases is increased, but the heavy fluid hydrocarbons are also formed. This mode of decomposition is evidently a mixed one, partaking of the characters of a regulated distillation, while at the same moment a more complete destruction of the coal is proceeding in some parts of the mass.

A further decomposition of the fluid products, condensed from either or both of these modes of operating, takes place when we again subject them to the influence of heat; and this well-known fact is the basis on which improvements in the manufacture of illuminating gas have been founded,—a secondary destruction of vapors being effected in appropriate apparatus, heated to a high temperature.

This character, which all the bituminous coals exhibit, of passing into carbon nearly free from vapors only when heavy fluid hydrocarbons are also formed, has, in a chemical view, been the strongest fact adduced in opposition to the generally received opinion that the anthracites and semi-anthracites have resulted from chemical changes of bituminous coal, through the agency of the heat of igneous rocks which have disturbed their beds. The heavy hydrocarbons, represented by ordinary coal tar, are the most indestructible bodies known; and wherever anthracites exist, we should expect to find near by those products of the chemical changes effected in the coal. Such is the delicacy of the balance existing between the elements of the heavy hydrocarbons, that no second distillation of them can be effected; they always undergo decomposition by heat, with the separation of carbon, which under any known natural conditions, would remain to attest their previous presence.

Considerations of this kind have led me to experiment on the changes which coals undergo by heat, where the influencing conditions were not the same as those usually seen; and the results of extended trials demonstrate that the bituminous coals may be broken up into permanent gases, vapors of water, and ammoniacal salts, while carbon remains as a fixed product.

If we substitute, for the ordinary forms of apparatus used in decomposing coal by heat suddenly applied, any modification of form which compels the gas, as it forms, to escape from the more highly heated part of the mass of coal, through a small opening, or, better, a small eduction pipe, the heavy hydrocarbons do not form part of the products which

escape. Generally the light, nearly colorless, oils of the benzole series appear with the aqueous solutions of the ammoniacal salts, while only an accidental quantity of carbon is deposited in the eduction-pipe. The carbon left is more than usually compact and hard; and such coals as ordinarily produce much water, when they form heavy hydrocarbons, afford less than half the usual amount, when thus decomposed, under the influence of the constant presence of an atmosphere of permanent gases.

In following the observations at the earlier stage, it was found that the size of the eduction-tube leading the gas from the hotter part of the mass of coal undergoing changes, exerted a most marked effect on the composition of the products. It was established as a fact, that in an ordinary coal-gas retort, the size of the conduit might be varied so as to allow the tar-like bodies to form, or to prevent their appearance at pleasure.

But a more remarkable result was obtained, when, after having prevented the production of heavy hydrocarbon fluids, the influence of reduced size of tube was studied in its relation to the composition of the gas afforded by a particular kind of coal. To a certain extent, the chemical constitution of the gas formed was found to be under control, and the conclusion reached was, that dissimilar permanent gases may be thus obtained from the same parcel of coal without a modification of temperature.

Any explanation of the change of composition induced in the volatile parts of bituminous coals under the above-described conditions should not include mechanical pressure, which is no greater than often exists in ordinary cases.

It seems probable that the presence of an atmosphere of nearly permanent gases in the decomposing vessel, and the regular continuous flow of them from the coal, prevent the formation of heavy vapors at the instant of change in the coal. In support of this point, we find the temperature necessary to convert coal into gas without the presence of heavy hydrocarbons much less high than when they are produced.

We may therefore observe the decomposition of coal without the simultaneous formation of tar, and *beds of coal may be converted under existing natural conditions to anthracite, without secondary products being formed.*

3. *Museum of Comparative Zoology in Harvard University.*—Since the connection of Professor Agassiz with the scientific department of Harvard University, he has been actively devoted, as is well known, to collecting zoological specimens and laying the foundation of a great museum. The collections already made by him or through his agency, and in great part at his own expense, are very large. The interest felt in this movement has been general through the country, and has recently taken a fresh start which is destined to lead to the most important results. The late Francis C. Gray of Boston—a gentleman extensively known for the depth and variety of his knowledge in many departments of literature, and for his liberal spirit in promoting schemes for the public good—was strongly attached to the study of the natural sciences, during the last years of his life, and was in habits of intimate and cordial association with Prof. Agassiz.

This distinguished gentleman died two years ago. He left by will his large, choice, and most valuable collection of engravings to Harvard College, together with a fund of sixteen thousand dollars to defray the expense of cataloguing and preserving them. But the most important of his legacies for public objects is that of fifty thousand dollars, as described in the extract from his will, quoted by William Gray, his nephew and executor, in a letter addressed to the corporation, dated Boston, Dec. 20, 1858, as follows:

"And also give, out of such surplus only, to Harvard College, or such other institution as you see fit, the further sum of fifty thousand dollars; the income to be applied to establishing and maintaining a Museum of Comparative Zoology; not to be appended to any other department, but to be under the charge of an independent Faculty, responsible only to the Corporation and Overseers. No part of said income is to be expended for real estate or the payment of salaries."

The conditions under which this donation has been bestowed on Harvard University are as follows:

"*First*, That the same be kept as a separate and distinct fund, and invested from time to time at the discretion of the Corporation, provided that no part thereof shall ever be invested in real estate, or in the shares or stock of any incorporated or joint-stock company.

"*Second*, No part of the income of said fund shall ever be expended for real estate or the payment of salaries.

"*Third*, The income is not to be subject to any charges of any nature, but the whole amount derived from the fund is to be applied to establishing and maintaining a Museum of Comparative Zoology at Harvard College.

"*Fourth*, Neither the collections, nor any building which may contain the same, shall ever be designated by any other name than the Museum of Comparative Zoology at Harvard College.

"*Fifth*, The Museum shall never be appended to any other department, but is to be under the charge of an independent Faculty, responsible only to the Corporation and Overseers.

"*Sixth*, The President of the College shall be the President of the Faculty, which shall be composed of four members besides the President. In case of vacancies in their number, other than that of President, the Faculty shall from time to time nominate to the Corporation persons to fill such vacancies; and if confirmed by the Corporation, such persons are to become members of the Faculty; if rejected, new nominations shall be made by the Faculty to the Corporation.

"*Seventh*, The Faculty are not to be at liberty to expend any part of the income of the fund, unless previously placed at their disposal by the Corporation.

"*Eighth*, The first Faculty shall consist of Rev. Dr. James Walker, President of the College, Professor Louis Agassiz, Director of the Museum, Dr. Jacob Bigelow, Professor Oliver Wendell Holmes, and Professor Jeffries Wyman.

"*Ninth*, In case of the loss of any part of the fund, so much of the income as may be requisite for this purpose shall be retained to make good such loss, provided that not more than one-half of the income shall be so retained in any one year.

"*Tenth*, That the Corporation enter this donation, with its conditions, upon their records, and vote to accept the same."

The conditions above prescribed are most judicious. The object for which the fund is established, in their nature cannot change. But there still remained other objects which imperatively demanded to be provided for. The collections of Professor Agassiz are at present in a small wooden building, which the torch of an incendiary might in an hour reduce to ashes, and thus annihilate the fruit of twelve years toil. Attention has been repeatedly called to this subject, and the announcement that Mr. William Gray, in the exercise of the discretion vested in him by his uncle's will, had decided to give the fifty thousand dollars for the support of the Museum, with the limitations already recited, seemed to be a sufficient reason for bringing the necessity of a fire-proof building again before the public. The matter was forcibly presented by Prof. Agassiz to the Visiting Committee of the Scientific School, in January. We give an extract from this document, which has been printed by order of the Overseers.

"I have laid out a plan which I will simply submit to you. I am afraid you will consider it extravagant, but if I understand rightly the aspirations of the young men with whom I am every day brought into contact, I cannot consider it over-sanguine, or doubt that the time is coming before long, when the scientific progress of the country will demand such an institution.

"My hope is that there shall arise upon the grounds of Harvard a Museum of Natural History, which shall compete with the British Museum and with the Jardin des Plantes. Do not say it cannot be done, for you cannot suppose that what exists in England and France cannot be reached in America. I hope even that we shall found a museum which will be based upon a more suitable foundation and better qualified to advance the highest interests of science than these institutions of the old world.

"But although I have sketched a plan for such a museum, I am nevertheless fully aware that, at the beginning, it must be carried out in a manner commensurate with the probable means that may be secured. Let us first erect a wing of that ideal museum, at an expense of perhaps $50,000, or, if that is too much, let us limit ourselves to such rooms as will give fitting shelter to the collections already on hand, and secure them from the danger of fire and other casualties. A spark of fire in this slight wooden building, where the collections are now heaped together, would be sufficient to destroy in half an hour the collections which it has cost me twelve years to amass, and which I can truly say is the most valuable collection for the student of natural history on this continent, and, in some of its classes, superior to any in the world. The mere possibility of unpacking what is already in our possession under this roof, crowded together in barrels and boxes, inaccessible to myself or the students, of displaying them to the public, and making them useful for study, will, I have not the slightest doubt, be a sufficient stimulus in the community to secure what will be needed to finish the structure and to render it worthy of the institution with which it will be connected, and the enlightened people who understand that in our age, culture is the

only true distinction among nations. One thing only should not be overlooked, that whenever any structure is put up for the museum, it should not be built in a corner where it cannot grow, but be placed on such grounds as will never be an impediment to its indefinite increase.

"In its present condition the museum hardly furnishes me the specimens I require for my courses of instruction, for, in consequence of the daily accessions which are heaped upon those already crowded in this narrow space, it is often impossible to find what is wanted at the time, and it is out of the question to allow free access to the Museum in its present confused state, to any student not already trained in the manipulation of specimens. Had I six or eight rooms of the size of the two now at my disposition, I could at least make a fair beginning of a systematic arrangement, separate the duplicates from what is to constitute the collection proper, allow free access to the rooms for the public as well as the students, and thus create a more general interest for this establishment, while the students themselves would derive all the advantages which such a collection ought to afford them in their studies. At the same time, the separation of the duplicates from the collection proper would furnish ample materials for an extensive system of exchanges with other institutions of the same kind, by which the collection would at once be at least doubled in all its parts, and in some of its departments increased three or four times, and in some, even tenfold. The advantages of such a system of exchanges are very obvious, and my inability from want of room to separate the duplicates from the collection, has already been, for some years past, a check upon its increase. I hope, therefore, that as soon as it is fully understood, some remedy for this evil may be found.

"But even the possession of an appropriate building will not altogether put an end to our difficulties. The collection is already so large that it is impossible for me to take charge of it alone, even were I to give all my time to its care. For many years past I have already been under the necessity of having one or two, and at times even three assistants, who, at my private expense, have been, most of the time, engaged in taking care of the specimens. As I have nothing in the world but what I earn daily, such an expenditure has frequently been for me a source of unendurable anxiety, of which I wish to free myself, that I may hereafter devote whatever energy I may possess untrammeled to the higher interests of science. In this perplexity I have thought that a number of curatorships, (corresponding to the scholarships now existing in the university, which enable young men, whose private means are insufficient for such an object, to receive a college education) might perhaps be founded by some of our wealthy citizens, which would furnish a small income to students who have already taken their degree, and who, wishing to prosecute further their studies under my direction, might thus earn the means of remaining in Cambridge by assisting in the arrangement and preservation of the collection, as well as in making the exchanges. The position of the curators in the scientific school would thus be similar to that of the tutors in the undergraduate department. In a well organized museum there should be as many curators as there are branches in zoology, including embryology, paleontology and zoological anthropology. In the

course of time, the curatorships (to which should be attached the duty of delivering a certain number of lectures annually) may be endowed so as to afford the means of appointing special professors for each branch, and as soon as this is accomplished, our organization would be more perfect than that of either the British Museum or the Jardin des Plantes. Beside the curators, there should be one or two preparators, to mount specimens, and to make the necessary preparations required for the illustration of the specimens. It would also be desirable to have an artist attached to the establishment, who would have to make magnified drawings of such specimens as are too small to be at once studied by the natural powers of the eye; these drawings would be appropriate ornaments for the corridors, and at the same time assist in the courses of lectures which it should be the duty of every curator to deliver annually upon the special branches intrusted to his care."

The very able chairman of the committee, the Hon. John H. Clifford, made an earnest report, urging upon the Overseers and the community the importance of acting at once upon the suggestions of Prof. Agassiz. The interest which these communications immediately excited, was great and general, and steps were taken to carry out, or at least to commence the execution of the plan. Several meetings of the most distinguished and enlightened citizens of Boston have been held, and liberal sums have already been promised. A general subscription has been undertaken, with the certainty of success. Not only this, but the subject has already been brought before the Legislature, and there is strong ground to believe that a handsome appropriation will be made, from the moneys received by the State from the sale of the "Back Bay" lands. Governor Banks, in his annual message, called attention, in general but emphatic terms, to the value of the natural sciences, and has since shown a liberal disposition to favor this particular measure. The Hon. Charles Hale, Speaker of the House of Representatives—and one of the most rising young men in Massachusetts—is also understood to be a warm friend of the proposal. Other leading persons in the government look upon it with favor, and there seems little doubt that a majority of the Legislature will take the same enlightened view. F.

4. *Observations on the Genus Unio*, together with descriptions of new species, their soft parts, and embryonic forms, in the family Unionidæ. 96 pages 4to, with 29 plates; by Isaac Lea, LL.D. (From the Journal of the Academy Nat. Sci. Philad., 1858).—These researches constitute a volume, of which the first paper was read in Dec. 1857, and the remaining two in November, 1858. The embryonic form of the shell in the case of 38 species of Unionidæ is figured without details on one of the plates. The prevalent form is pouch-shaped, the height much greater than the length. In the last paper on new Unionidæ of the United States, numerous species are described and well figured. Dr. Lea observes that he has found the *Unio cylindricus* Say, *U. rubiginosus* Lea, and *Anodonta imbecilis* Say, sensitive to light, as if possessing some kind of visual organs, and that Prof. Haldeman had observed and published the same for the *Unio radiatus*. Mr. Lea's first publication on this point was in the Proceedings Acad. Nat. Sci. Philadelphia for February, 1857.

5. *On the Stratification of Vesicular Ice by Pressure;* by Prof. WILLIAM THOMSON, F.R.S., in a letter to Prof. STOKES, Sec. R.S., (Proc. Roy. Soc. from Phil. Mag., Dec. 1858.)—In my last letter to you I pointed out that my brother's theory of the effect of pressure in lowering the freezing point of water, affords a perfect explanation of various remarkable phenomena involving the internal melting of ice, described by Prof. Tyndall in the number of the "Proceedings" which has just been published. I wish now to show that the stratification of vesicular ice by pressure observed on a large scale in glaciers, and the lamination of clear ice described by Dr. Tyndall as produced in hand specimens by a Brahmah's press, are also demonstrable as conclusions from the same theory.

Conceive a continuous mass of ice, with vesicles containing either air or water distributed through it; and let this mass be pressed together by opposing forces on two opposite sides of it. The vesicles will gradually become arranged in strata perpendicular to the lines of pressure, *because of the melting of ice in the localities of greatest pressure and the regulation of the water in the localities of least pressure, in the neighborhood of groups of these cavities.* For, any two vesicles nearly in the direction of the condensation will afford to the ice between them a relief from pressure, and will occasion an aggravated pressure in the ice round each of them in the places farthest out from the line joining their centres; while the pressure in the ice on the far sides of the two vesicles will be somewhat diminished from what it would be were their cavities filled up with the solid, although not nearly as much diminished as it is in the ice between the two. Hence, as demonstrated by my brother's theory and my own experiment, the melting temperature of the ice round each vesicle will be highest on its side nearest to the other vesicle, and lowest in the localities on the whole farthest from the line joining the centres. Therefore, ice will melt from these last-mentioned localities, and, if each vesicle have water in it, the partition between the two will thicken by freezing on each side of it. Any two vesicles, on the other hand, which are nearly in a line perpendicular to the direction of pressure will agree in leaving an aggravated pressure to be borne by the solid between them, and will each direct away some of the pressure from the portions of the solid next itself on the two sides farthest from the plane through the centres, perpendicular to the line of pressure. This will give rise to an increase of pressure on the whole in the solid all round the two cavities, and nearly in the plane perpendicular to the pressure, although nowhere else so much as in the part between them. Hence these two vesicles will gradually extend towards one another by the melting of the intervening ice, and each will become flattened in towards the plane through the centres perpendicular to the direction of pressure, by the freezing of water on the parts of the bounding surface farthest from this plane. It may be similarly shown that two vesicles in a line oblique to that of condensation will give rise to such variations of pressure in the solid in their neighborhood, as to make them, by melting and freezing, to extend, each obliquely *towards* the other and *from* the parts of its boundary most remote from a plane midway between them, perpendicular to the direction of pressure.

The general tendency clearly is for the vesicles to become flattened and arranged in layers, in planes perpendicular to the direction of the pressure from without.

It is clear that the same general tendency must be experienced even when there are bubbles of air in the vesicles, although no doubt the resultant effect would be to some extent influenced by the running down of water to the lowest part of each cavity.

I believe it will be found that these principles afford a satisfactory physical explanation of the origin of that beautiful veined structure which Prof. Forbes has shown to be an essential organic property of glaciers. Thus the first effect of pressure not equal in all directions, on a mass of snow, ought to be, according to the theory, to convert it into a stratified mass of layers of alternately clear and vesicular ice, perpendicular to the direction of maximum pressure. In his remarks "On the Conversion of the Névé into ice,"* Prof. Forbes says, "*that the conversion into ice is simultaneous*" (and in a particular case referred to "*identical*") "*with the formation of the blue bands;* . . . . and that these bands are formed where the pressure is most intense, and where the differential motion of the parts is a maximum, that is, near the walls of a glacier." He further states, that, after long doubt, he feels satisfied that the conversion of snow into ice is due to the effects of pressure on the loose and porous structure of the former; and he formally abandons the notion that the blue veins are due to the freezing of infiltrated water, or to any other cause than the kneading action of pressure. All the observations he describes seems to be in most complete accordance with the theory indicated above. Thus, in the thirteenth letter, he says, "the blue veins are formed where the pressure is most intense and the differential motion of the parts a maximum."

Now the theory not only requires pressure, but requires difference of pressure in different directions to explain the stratification of the vesicles. Difference of pressure in different directions produces the "differential motion" referred to by Professor Forbes. Further, the difference of pressure in different directions must be continued until a very considerable amount of this differential motion, or distortion, has taken place, to produce any sensible degree of stratification in the vesicles. The absolute amount of distortion experienced by any portion of the viscous mass is therefore an index of the persistence of the differential pressure, by the continued action of which the blue veins are induced. Hence also we see why blue veins are not formed in any mass, ever so deep, of snow *resting* in a hollow or corner. * * *

6. *Thoughts on Matter and Force, or marvels that encompass us, comprising suggestions illustrative of the theory of the earth and the universe;* by Thomas Ewbank, author of "Hydraulics and Mechanics," &c. 154 pp. 18mo. New York, 1858.—There are many excellent thoughts in this little work. But we cannot subscribe to its main doctrines, that expansion is the only effective moving power in a forming earth; that the earth's heat is due to pressure from gravitation; and that gravitation is a means of preventing any decrease of the mean temperature of a globe

* Thirteenth Letter on Glaciers, section (2), dated Dec. 1846.

because heat is generated by pressure and by friction attending the circulation of the hot liquid. Expansion and contraction for a given change of temperature are equal in amount and also in mechanical power, and both may be sources of movements in the earth's crust. As regards the circulating liquid, the power causing the circulation is equivalent in amount of heat expended to the heat produced by the friction in the circulation, so that there can be no increase from this source. Since gravitation can produce no condensation except there be a loss of heat, it cannot be a means of augmenting the heat; and hence, whatever may be the amount at any moment, the sphere will still lose by radiation, and find its only possible means of compensation for the loss in external sources. The author touches on geological topics while no geologist, and therefore without being aware of the points that are to be met in the required explanations.

7. *Shower of Mud at Corfu.*—Dr. G. Lawson describes a mud shower as having occurred at Corfu on the 21st of March, 1857. The day was squally and showery, and with the rain came down a light shower of mud which covered lightly the leaves of the trees and garden plants. Under microscopic examination the earthy material was found to consist mainly of quartz grains and not of minute organisms.

8. *Notes on American Land Shells, No.* 4; by W. G. Binney, (Proc. Acad. Nat. Sci. Philad., 1858, Nov.)—Mr. Binney in this paper gives a catalogue of American Terrestrial Mollusks, with full lists of synonyms, which will be found of great convenience to all interested in this subject.

9. *Memoirs of the Geological Society of Great Britain, and of the Museum of Practical Geology.* The Iron Ores of Great Britain. Part II, The Iron Ores of South Staffordshire; by J. Beale Jukes, with various analyses, made under the direction of Dr. Percy. 64 pp. 8vo. London, 1858.—The South Staffordshire iron works occur in an area of 50 square leagues about Dudley.

10. *Smithsonian Report for* 1857.—The Smithsonian Institution is doing for science what is done by a Royal Society abroad; and much more. For besides publishing elaborate papers and works which would fail of a publisher on account of the expense, it is giving activity to science over the land,—calling out zealous research, and full collections of observations and specimens, in explorations over the Rocky Mountains and the regions west,—making a gallery of Indian portraits, the Stanley collection being deposited there, with the prospect of its being purchased by government,—gathering a cabinet of the natural productions of the United States,—collecting a library of all the Transactions of foreign Societies and Journals, which is already remarkably complete and nearly as possible up to the date of publication,—eliciting and issuing Reports on different departments of science,—sustaining a series of lectures at Washington through the winter,—and making itself, for the great convenience of the country, a medium of communication in the way of publications, between the science of the two Continents. This Report gives a brief account of the publications and encouragements of researches and general progress of the Institution the past year; also lectures by Prof. Joseph LeConte and Prof. S. Alexander, communications on Meteorology, and a Translation of Dr. J. Müller's very valuable Report of recent progress in Physics, made by G. C. Schæffer.

It contains also the decision of the Board of Regents upon the charges brought by Prof. S. F. B. Morse (contained in Shaffner's Telegraph Companion in 1855) against Prof. Henry, implying his "consciously and willfully deviating from the truth and this too from unworthy and dishonorable motives" in his testimony respecting the claims of Mr. Morse touching the origin of the electro-magnetic telegraph. The charges were of so gross a character, that Professor Henry, under a sense of the responsibilities of his position as Secretary, deemed it incumbent on him to bring them before the Board. The report of the Committee of investigation pronounces the charges unsustained. Prof. Henry follows the report with a history of the discoveries that prepared the way for the telegraph.

11. *Straw Lightning rods.*—The power of straw as a conductor of electricity has been utilized in the south of France, no less than eighteen Communes in the neighborhood of Tarbes having been provided with conductors composed of straw. Experiments show that an electrical shock sufficiently powerful to kill an ox may be discharged by a single straw.—*Athen., No.* 1630.

12. *Tschudi.*—Dr. Tschudi, the well-known traveller, has just returned from his last journey to Peru; the results of which will be submitted shortly to the public.—*Ib.*

Obituary.—*Death of William C. Bond.*—We are pained to announce the death of William Cranch Bond, Esq., the director of the Astronomical Observatory of Harvard College. He died at Cambridge, Mass., Jan. 29, 1859, aged 69. He was born in Portland, Me., Sept. 9, 1789. Before his appointment to the Cambridge Observatory, he had devoted himself with much industry, talent and success, not only to astronomical observations, but to the construction and improvement of optical instruments, in every detail of which he was well informed and practically skillful. Having gained a reputation as an observer at his private observatory in Dorchester, he was called to the charge of that in Cambridge, in 1839, before any buildings were erected. The great telescope was mounted June 24, 1847. In connection with his sons, he has used that great refractor with important results, in observations of the fixed stars, the nebulæ, and the planet Saturn. To his practical skill observers owe a piece of mechanism, called the "Spring Governor," by which time is visibly measured to a small fraction of a second. To the same skill in applying scientific knowledge to mechanical means was in a large part owing what is known in Europe as the "American method" of recording astronomical observations by electro-magnetism. He has been engaged with encouraging success in experiments for taking photographs of the stars by a camera attached to the great telescope. Before his appointment at Cambridge he was employed by the U. S. Government in making astronomical observations in connection with those of the South Sea Exploring Expedition. Mr. Bond's talents and acquirements as a skillful astronomer were duly appreciated not only in this country but also in Europe. In 1842 the honorary degree of Master of Arts was conferred on him by Harvard College. He was a member of the American Academy of Arts and Sciences, of the American Philosophical Society, and of the Royal Astronomical Society of London. By his death the College is deprived of a highly valued officer, and the scientific community of one of its most gifted and accomplished sons.—*Boston Daily Advertiser.*

Synopsis of the Report on Zoophytes; by JAMES D. DANA. 180 pp. 8vo. New Haven, 1859; containing descriptions of all the species in the Author's original 4to. Report, which is out of print.

H. L. BOWDITCH: Address on the Life and Character of James Deane, M.D., of Greenfield, Mass. 46 pp. 8vo.

Lieut. J. C. IVES: Colorado Exploring Expedition, Preliminary Report to Captain A. A. Humphreys, Topograph. Engineers. 12 pp. 8vo.

J. CASSIN: Mammalogy and Ornithology of the United States Exploring Expedition under Captain Wilkes, U. S. N., 1838–1842. 1 vol. 4to in 466 pages, with a folio atlas of 53 colored plates, 11 of mammals and 42 of birds.

F. T. CONINGTON: Handbook of Chemical Analysis. London: Longman & Co.—The author adopts the notation of the Laurent school.

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